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GEOLOGY,
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NUMBER CLXXVII.
For JANUARY 1813.

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NUMBER CLXXVIII.

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1. Mr. Goss's Instrument to work Addition of Numbers with Accuracy and Dispatch.
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A Quarto Plate to illustrate Mr. J. Hicks's Account of the Situation and Means of raising the Royal George.

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2. A Plate to illustrate Mr. STEEVENS'S Account of a Meteor seen at London and other Places in March last.

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A Plate to illustrate Mr. Machell's Annular Saw.

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" Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

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OF THE

FORTY-FIRST VOLUME.

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THE

PHILOSOPHICAL MAGAZINE.

I. *An Attempt to determine the definite and simple Proportions, in which the constituent Parts of unorganic Substances are united with each other. By* JACOB BERZELIUS, *Professor of Medicine and Pharmacy, and M.R.A. Stockholm.*

[From GILBERT's Journal, 1811, iii. . . . Translated from a copy corrected by the Author expressly for this Work.]

PART FIRST.

MR. BERTHOLLET, one of the most celebrated chemists of our times, has endeavoured to demonstrate, in his ingenious researches respecting the laws of chemical affinities, that elementary substances may unite with each other in an infinite number of progressive proportions. Mr. Proust, however, another great master of the science, has shown, in opposition to him, that no such infinite variety of progressions is to be found in nature: but that all compound and precisely characterized bodies exhibit only a single and invariable proportion between their component parts; and that when a protoxide, for example, is converted by an additional portion of one of its component parts, that is of oxygen, into an oxide, this happens *per saltum*, proceeding at once to another precisely determined proportion, so that any continued series of combinations between these limits is out of the question. The truth of Proust's remark cannot have escaped any experienced chemist: but it has not hitherto been ascertained whether these distinct steps or stages of combination follow one and the same law for substances of all kinds, or whether the proportions are indeterminate, and different for different

 substances.

substances. The experiments, which I shall here relate, will prove that certain fixed laws prevail in all such cases.

I have been led to this investigation by attempting to deduce from calculation the quantity of oxygen contained in ammonia; on this occasion I discovered that the quantity of any base, by which a certain quantity of the muriatic acid is saturated, contains always the same quantity of oxygen: although in reality the merit of this discovery is due to Richter, who has endeavoured to demonstrate the principle, in the sixth part of his Essays, by some well imagined, though not fully satisfactory experiments, which have led him to adopt a series of numbers agreeing tolerably well with each other, but by no means perfectly accurate. The same law was observable in the sulphates, when Bucholz's analysis of the sulphate of baryta was made the basis of the calculation. There was however some disagreement in the two series; nor were the results consistent with other experiments; and it was necessary to take for granted the truth of the analysis of the muriate of silver instituted by Bucholz and Rose. I also found that, in the submuriates of lead and copper, the acid is combined with four times as much of the base as in the neutral salts.

I was in hopes of being able to discover the general principle of these remarkable relations by a correct investigation of the combinations of a variety of other similar substances. In the mean time I received a copy of Nicholson's Journal for November 1808, which contained an account of Wollaston's experiments on acid salts or supersalts, which had been suggested by the hypothesis of Dalton, that *when bodies are capable of being combined in different proportions, these proportions may always be expressed by multiplying the weight of one of the bodies by* 1, 2, 3, 4, *and so forth:* and Wollaston's experiments seemed to confirm the hypothesis. This view of the combinations of bodies appeared capable of illustrating so greatly the doctrine of affinity, that the confirmation of Dalton's hypothesis seemed to be the greatest step that chemistry, as a science, would have made during the whole time of its existence. On what experiments Dalton had founded his proposition, and in what manner he had extended its application, I am wholly ignorant; I cannot therefore determine whether my experiments simply confirm this hypothesis in its whole extent, or whether they have any tendency to modify it in any of its parts.

It will be proved by the following experiments, that *when two bodies,* A and B, *combine with each other in different*

ferent proportions, their respective quantities are always indicated by some of these simple expressions: 1 *A with* 1 *B*, the minimum, 1 *A with* $1\frac{1}{2}$ *B*, or perhaps rather 2 A with 3 B, 1 A with 2 B, *and* 1 *A with* 4 *B*. But there is no example in my experiments of the proportion 1 A with 3 B.

It will further be shown, that *when two bodies, A and B, have both affinities with two others, C and D, the quantities of C and D which are saturated by A, are precisely in the same proportion as the quantities which are saturated by B.* Thus, since 100 parts of lead are capable of combining in the first degree with 15·6 of sulphur and 7·8 of oxygen, and 100 parts of iron, according to the analysis which will hereafter be related, combine in the first degree with 58·8 of sulphur, the composition of the protoxide of iron may be computed by the simple proportion 15·6 : 7·8=58·8 : 29·4, so that 100 parts of iron require to be combined with 29·4 of oxygen. This is confirmed by the experiments which I am about to communicate, and all binary combinations may be determined in the same manner. It has been ably demonstrated by Richter, that a similar principle is applicable to the combinations of salts.

It is obvious that the result of these calculations, supposing them to be well founded, must be susceptible of much greater accuracy and certainty than the common analysis. I have endeavoured to give the greatest possible precision to the analyses which I shall here relate, and I have repeated the most important of them more than once, before I allowed myself to depend on them. These are certainly free from errors of more than one or two parts in a thousand, and the others are within at most one half per cent. of the truth, but still only accurate enough to give approximations in computation. Perhaps we shall never succeed in analysing substances so accurately, as to obtain results agreeing with the proportions of the component parts to the last place of decimals: on the other hand, it will not be impossible, when we have a number of very accurate analyses, to correct them so by calculation, that all the elements of the computation of a combination may afford precisely the same result.

I shall arrange my experiments in the order which seems most convenient for the illustration of the subject, and I shall totally refrain from all theorizing. How far the results of the experiments confirm the theory, will be obvious without particular comment, and the ideas to which they lead

 will

will naturally occur to every attentive reader without my assistance.

I. Lead and Oxygen.

Lead, as is well known, affords three oxides. In order to ascertain the proportion of oxygen contained in them, I employed lead which was obtained by the reduction of the crystallized nitrate, and which was consequently free from any mixture of copper or silver.

A. *Yellow Oxide of Lead.*

1.) Ten grammes of lead were dissolved in pure nitric acid, and in order to avoid loss, the process was performed in a flask or receiver of glass held in an inclined position. The solution was poured into a weighed dish of platina, carefully evaporated, and exposed to a red heat. It afforded 10·77 grammes of oxide.

2.) The experiment was repeated, but the evaporation and ignition were performed in the same vessel which served for the solution. The result was 10·775 grammes of oxide of lead.

3.) In a third experiment a flask with a long neck was employed. When the salt began to be decomposed, a small quantity of a mealy sublimate attached itself for an instant to the neck of the vessel, and the vapours had not the smell of a perfectly pure nitric acid. When the flask had been heated throughout its whole length, the weight of the oxidated lead amounted to 10·78 grammes, or a little more than in the former experiments; and at the same time an appearance had taken place in this experiment, which showed that a small portion of the oxide of lead was carried off with the vapour of the acid which was expelled.

4.) Ten grammes of lead were dissolved in nitric acid, and precipitated by carbonated ammonia: the precipitate was placed on a weighed filter and well washed. It amounted to 12·9025 grammes of carbonated lead. Of this 12·77 grammes were ignited in a dish of platina; the residuum was 10·64 gr. of yellow oxide of lead, giving 10·75 for the whole quantity; so that 100 parts of lead had taken up 10·75 of oxygen. I conceived a suspicion that the carbonated ammonia might not have thrown down the whole quantity of lead; I therefore passed sulphurated hydrogen through the liquor of precipitation, and through the water with which the precipitate had been washed; but they were not rendered turbid by it in the slightest degree.

5.) The

5.) The experiment was repeated with 8 grammes of lead, which afforded 10·32 of carbonate and 8·8 of yellow oxide of lead; so that 100 parts of lead had again taken up $7\frac{1}{2}$ of oxygen.

Bucholz obtained from 300 grains of lead, which were dissolved in nitric acid, and precipitated by carbonated alkali, 320 grains of yellow oxide of lead; besides $4\frac{1}{2}$ grains of carbonated lead, which remained on the filter. This last is taken by Bucholz, as equivalent to 4 grains of the yellow oxide: this however is an error; the carbonate of lead loses $\frac{1}{6}$ of its weight, not $\frac{1}{9}$ only, by ignition; for 10 grammes of pure carbonate of lead, dried in a strong heat, afforded me, in three different experiments, 8·35 gr. of yellow oxide, so that we must allow only $3\frac{3}{4}$ grains for the $4\frac{1}{2}$, and the lead in Bucholz's experiment cannot have taken up more than 7·92 per cent. of oxygen.

From these experiments I think myself authorised to conclude, that those are the most accurate which give the proportion of oxygen from 7·75 to 7·8 for 100 of lead. Consequently the yellow oxide or protoxide of lead consists of

Lead	92·764	or	100·0
Oxygen	7·236		7·8
	100·000		107·8

B. *Red Oxide of Lead. Red Lead.*

Red lead, as it occurs in commerce, I have found contaminated with sulphate and submuriate of lead, oxide of copper, and silica: so that little dependence can be placed on its analysis. It also contains much of the yellow oxide, which gives it a brighter colour than properly belongs to lead in this stage of oxidation.

In order to get rid of the yellow oxide, I digested some levigated red lead with weak distilled vinegar, at a temperature of 68°, as long as the vinegar continued to saturate itself: by these means the yellow oxide was dissolved, while the red remained unaltered, the colour only becoming deeper. After washing and drying in a very strong heat, 10 grammes of this red lead were ignited in a weighed platina dish; they lost ·29 gr. The oxide, which had become yellow, was now dissolved in vinegar; the sulphate of lead and silica which were left in this process, weighed when ignited ·135 gr. To the solution in vinegar nitrate of silver was added, and ·01 gr. of muriate of silver was precipitated, which answers to ·03 gr. of submuriate of lead; so that in the whole there was ·165 of foreign matter. Consequently

 9·835 gr.

9·835 gr. of red lead had afforded ·29 of oxygen and 9·545 of yellow oxide, or 8·855 of lead, which had been united with ·98 of oxygen. Now 8·855 : ·98 = 100 : 11·07; consequently 100 parts of lead, in becoming red lead, take up 11·07 of oxygen, and this oxide consists of 90 parts of lead and 10 of oxygen.

C. *Brown Oxide of Lead.*

It is well known that red lead, digested with nitric acid, affords a brown oxide of lead. While the nitric acid dissolves the yellow oxide and reduces a part of the red to yellow, it leaves the brown oxide undissolved, together with a quantity of impurities, especially sulphate of lead and silica.

Five grammes of brown oxide of lead, freed by washing from all the nitrate which had adhered to it, and dried in a sand-bath, with a heat capable of melting tin, were ignited in a weighed dish of platina, and lost ·325 gr. of oxygen. The remaining 4·675 gr. of yellow oxide, dissolved in vinegar, left behind some sulphate of lead and silica, which when ignited weighed together ·13 gr. The remaining 4·545 gr. of yellow oxide contain ·33 gr. of oxygen, a quantity differing only by ·005 gr. from that which the brown oxide had lost by ignition. Consequently 100 parts of lead, in order to be converted into brown oxide, require twice as much oxygen as is contained in the yellow oxide, and the brown oxide consists of

Lead	86·51	100·0
	13·49	15·6
	100·00	115·6

It seems to follow as the result of these experiments, that lead, in its three different degrees of oxidation, takes up oxygen in quantities which are related in the proportions of 1, $1\frac{1}{2}$, and 2.

[To be continued.]

II. *Derivation of one of the Equations in* LAPLACE'S "*Mécanique Céleste.*"

To Mr. Tilloch.

DEAR SIR, THE accompanying paper I had from my particular friend and successor in the mathematical school of Dumfries, Mr. Thomas White. That school I established about 40 years ago, and Mr. White has taught it with

with much credit to himself and utility to the public during the last 30 years. The object of the paper is sufficiently explained in his short letter prefixed to the calculation. You will oblige us, and I doubt not many other of your mathematical readers, by inserting it in the Philosophical Magazine.

Yours very truly,

JAMES DINWIDDIE.

To Dr. Dinwiddie.

DEAR SIR,—IN the *Mécanique Céleste*, vol. i. page 138, Laplace has given an equation marked (B) which is of great use in the theory of the figure of the celestial bodies. To the young reader of that profound work the derivation of the above equation may be acceptable; and, if you think that Mr. Tilloch will allow it a place in his Magazine, it is at his service.

Yours sincerely,

Dumfries Mathematical School, Nov. 9, 1812. THOMAS WHITE.

In Laplace's equation $\left(\frac{d^2V}{dx^2}\right) + \left(\frac{d^2V}{dy^2}\right) + \left(\frac{d^2V}{dz^2}\right) = 0$, marked (A); V is a function of x, y, and z; and x is $= r.\cos\theta$; $y = r.\sin\theta.\cos\pi$; and $z = r.\sin\theta.\sin\pi$; and, therefore, $r = \sqrt{x^2+y^2+z^2}$; $\cos\theta = \frac{x}{\sqrt{x^2+y^2+z^2}}$; and, the tang $\pi = \frac{z}{y}$. Hence,

$$\left(\frac{dr}{dx}\right) = \frac{x}{r} = \cos\theta.$$

$$\left(\frac{d\theta}{dx}\right) = -\frac{\sin\theta}{r}; \text{ because, } -\sin\theta.\left(\frac{d\theta}{dx}\right) = \frac{d.\cos\theta}{dx} = \frac{d\left(\frac{x}{r}\right)}{dx} = \frac{1}{r} - \frac{x}{r^2}.\left(\frac{dr}{dx}\right).$$

$$\left(\frac{d\pi}{dx}\right) = 0. \text{ Therefore,}$$

$$\left(\frac{d^2r}{dx^2}\right) = \frac{\sin^2\theta}{r}; \text{ since } \frac{d^2r}{dx^2} = \frac{d\left(\frac{dr}{dx}\right)}{dx} = \frac{d.\cos\theta}{dx} = -\sin\theta \times \left(\frac{d\theta}{dx}\right) = \frac{\sin^2\theta}{r}.$$

$$\left(\frac{d^2\theta}{dx^2}\right) = \frac{2.\sin\theta.\cos\theta}{r^2}; \text{ for, } \frac{d.\left(\frac{d\theta}{dx}\right)}{dx} = \frac{-d\left(\frac{\sin\theta}{r}\right)}{dx} = \frac{-1}{r}.$$

$$\frac{-1}{r}\cdot\frac{d.\sin\theta}{dx}+\frac{\sin\theta}{r^2}\cdot\left(\frac{dr}{dx}\right)=\frac{\cos\theta}{r}\cdot\left(\frac{d\theta}{dx}\right)+\frac{\sin\theta}{r^2}\cdot\left(\frac{dr}{dx}\right)$$
$$=\frac{2.\sin\theta.\cos\theta}{r^2}.$$

$\left(\frac{d^2\pi}{dx^2}\right)=0.$ Again,

$$\left(\frac{dr}{dy}\right)=\frac{y}{r}=\sin\theta.\cos\pi.$$

$\left(\frac{d\theta}{dy}\right)=\frac{\cos\theta.\cos\pi.}{r}$; for, $\frac{d.\cos\theta}{dy}=\frac{d\left(\frac{x}{r}\right)}{dy}$; that is,
$$\sin\theta.\left(\frac{d\theta}{dy}\right)=\frac{x}{r^2}\cdot\left(\frac{dr}{dy}\right)=\frac{\sin\theta.\cos\theta.\cos\pi}{r}.$$

$\left(\frac{d\pi}{dy}\right)=\frac{-\sin\pi}{r.\sin\theta}$; for, $\frac{d\left(\frac{z}{y}\right)}{dy}=\frac{d.\tan g\,\pi}{dy}=\frac{d.\left(\frac{\sin\pi}{\cos\pi}\right)}{dy}=$
$\frac{1}{\cos^2\pi}\cdot\left(\frac{d\pi}{dy}\right)$; *i.e.* $\frac{1}{\cos^2\pi}\cdot\left(\frac{d\pi}{dy}\right)=-\frac{z}{y^2}=$
$\frac{-\sin\pi}{r.\sin\theta.\cos^2\pi}.$

$\left(\frac{d^2r}{dy^2}\right)=\frac{\sin^2\pi+\cos^2\pi.\cos^2\theta}{r}$; for, $\frac{d\left(\frac{dr}{dy}\right)}{dy}=\frac{d.(\sin\theta.\cos\pi)}{dy}=$
$\sin\theta.\frac{d\cos\pi}{dy}+\cos\pi.\frac{d.\sin\theta}{dy}=-\sin\theta.\sin\pi\cdot\left(\frac{d\pi}{dy}\right)$
$+\cos\pi.\cos\theta.\left(\frac{d\theta}{dy}\right)=\frac{\sin^2\pi+\cos^2\pi.\cos^2\theta}{r}.$

$\left(\frac{d^2\theta}{dy^2}\right)=\frac{\cos\theta}{r^2\sin\theta}.[\sin^2\pi-2.\sin^2\theta.\cos^2\pi]$; for, $\frac{d\left(\frac{d\theta}{dy}\right)}{dy}$
$=\frac{d.\left(\frac{\cos\theta.\cos\pi}{r}\right)}{dy}=\frac{\cos\theta}{r}\cdot\frac{d.\cos\pi}{dy}+\frac{\cos\pi}{r}.\frac{d.\cos\theta}{dy}$
$-\frac{\cos\theta.\cos\pi}{r^2}.\left(\frac{dr}{dy}\right)=-\frac{\cos\theta.\sin\pi}{r}.\left(\frac{d\pi}{dy}\right)-$
$\frac{\sin\theta.\cos\pi}{r}.\left(\frac{d\theta}{dy}\right)-\frac{\cos\theta.\cos\pi}{r^2}.\left(\frac{dr}{dy}\right)=\frac{\cos\theta}{r^2.\sin\theta}\times$
$[\sin^2\pi-2.\sin^2\theta.\cos^2\pi].$

$\left(\frac{d^2\pi}{dy^2}\right)=\frac{2.\sin\pi.\cos\pi}{r^2.\sin^2\theta}$; for, $\frac{d\left(\frac{d\pi}{dy}\right)}{dy}=\frac{d\left(\frac{\sin\pi}{\cos\pi}\right)}{dy}=\frac{-\cos\pi}{r.\sin\theta}\times$
$\left(\frac{d\pi}{dy}\right)+\frac{\sin\pi.\cos\theta}{r.\sin^2\theta}.\left(\frac{d\theta}{dy}\right)+\frac{\sin\pi}{r^2.\sin\theta}.\left(\frac{dr}{dy}\right).$

Again,

$$\left(\frac{dr}{dz}\right)=\frac{z}{r}=\sin\theta.\sin\pi.$$

$\left(\frac{d\theta}{dz}\right) = \frac{\cos\theta \,.\, \sin\varpi}{r}$; for, $\left(\frac{d\theta}{dz}\right) = \frac{-1}{\sin\theta} \,.\, \frac{d\,.\cos\theta}{dz} = \frac{x}{r^2 .\sin\theta} \times \left(\frac{dr}{dz}\right)$.

$\left(\frac{d\varpi}{dz}\right) = \frac{\cos\varpi}{r \,.\, \sin\theta}$; since $\frac{d\left(\frac{z}{y}\right)}{dz} = \frac{d\,.\,\text{tang}\,.\,\varpi}{dz} = \frac{d\left(\frac{\sin\varpi}{\cos\varpi}\right)}{dz} = \frac{1}{\cos\varpi} \,.\, \frac{d\,.\sin\varpi}{dz} - \frac{\sin\varpi}{\cos^2\varpi} \,.\, \frac{d\,.\cos\varpi}{dz} = \left(\frac{d\varpi}{dz}\right) + \frac{\sin^2\varpi}{\cos^2\varpi} \times \left(\frac{d\varpi}{dz}\right)$.

$\left(\frac{d^2 r}{dz^2}\right) = \frac{1}{r} \,.\, [1 - \sin^2\pi + \cos^2\theta \,.\, \sin^2\pi] = \frac{1}{r} \,.\, [1 - \sin^2\pi \,.\, \sin^2\theta]$; for, $\left(\frac{d^2 r}{dz^2}\right) = \frac{d\,.\,(\sin\theta \,.\, \sin\varpi)}{dz} = \sin\theta \,.\, \cos\pi \,.\, \left(\frac{d\varpi}{dz}\right) + \sin\pi \,.\, \cos\theta \,.\, \left(\frac{d\theta}{dz}\right)$.

$\left(\frac{d^2\theta}{dz^2}\right) = \frac{\cos\theta}{r^2 .\sin\theta} \,.\, [\cos^2\pi - 2 \,.\, \sin^2\theta \,.\, \sin^2\pi]$; for, $\left(\frac{d^2\theta}{dz^2}\right) = \frac{d\left(\frac{\cos\theta \,.\, \sin\varpi}{r}\right)}{dz} = \frac{\cos\theta \,.\, \cos\varpi}{r} \,.\, \left(\frac{d\varpi}{dz}\right) - \frac{\sin\varpi .\sin\theta}{r} \times \left(\frac{d\theta}{dz}\right) - \frac{\cos\theta \,.\, \sin\varpi}{r^0} \,.\, \left(\frac{dr}{dz}\right)$.

Also,

$\left(\frac{d^2\varpi}{dz^2}\right) = \frac{-2 \,.\, \sin\varpi \,.\, \cos\varpi}{r^2 .\sin^2\theta}$; since $\frac{d\left(\frac{d\varpi}{dz}\right)}{dz} = \frac{d\left(\frac{\cos\varpi}{r .\sin\theta}\right)}{dz} = \frac{-\sin\varpi}{r .\sin\theta} \,.\, \left(\frac{d\varpi}{dz}\right) - \frac{\cos\varpi \,.\, \cos\theta}{r .\sin^2\theta} \,.\, \left(\frac{d\theta}{dx}\right) - \frac{\cos\varpi}{r^2 .\sin\theta} \times \left(\frac{dr}{dz}\right)$.

Now, I. $\left(\frac{dV}{dx}\right) = \left(\frac{dV}{dr}\right) \,.\, \left(\frac{dr}{dx}\right) + \left(\frac{dV}{d\theta}\right) \,.\, \left(\frac{d\theta}{dx}\right)$; for, $\left(\frac{d\varpi}{dx}\right)$ is $= 0$. Hence,

$$\frac{d.\left(\frac{dV}{dx}\right)}{dx} = \left(\frac{d^2 V}{dr^2}\right) = \left(\frac{dV}{dr}\right) \,.\, \frac{d\left(\frac{dr}{dx}\right)}{dx} + \left(\frac{dr}{dx}\right) \,.\, \frac{d\left(\frac{dV}{dr}\right)}{dx} + \left(\frac{dV}{d\theta}\right) \,.\, \frac{d\left(\frac{d\theta}{dx}\right)}{dx} + \left(\frac{d\theta}{dx}\right) \,.\, \frac{d\left(\frac{dV}{d\theta}\right)}{dx};$$ that is,

$d^2 V$

$$\left(\frac{d^2 V}{dx^2}\right) = \left\{\begin{array}{l} \left(\frac{dV}{dr}\right) \cdot \left(\frac{d^2 r}{dx^2}\right) + \left(\frac{dr}{dx}\right) \cdot \left[\left(\frac{d^2 V}{dr^2}\right) \cdot \left(\frac{dr}{dx}\right) + \right. \\ \qquad \left. \left(\frac{d^2 V}{dr.\,d\theta}\right) \cdot \left(\frac{d\theta}{dx}\right)\right] + \\ \left(\frac{dV}{d\theta}\right) \cdot \left(\frac{d^2 \theta}{dx^2}\right) + \left(\frac{d\theta}{dx}\right) \cdot \left[\left(\frac{d^2 V}{d\theta.dr}\right) \cdot \left(\frac{dr}{dx}\right) + \right. \\ \qquad \left. \left(\frac{d^2 V}{d\theta^2}\right) \cdot \left(\frac{d\theta}{dx}\right)\right]. \end{array}\right.$$

Again,

$$\text{II. } \left(\frac{dV}{dy}\right) = \left(\frac{dV}{dr}\right) \cdot \left(\frac{dr}{dy}\right) + \left(\frac{dV}{d\theta}\right) \cdot \left(\frac{d\theta}{dy}\right) + \left(\frac{dV}{d\pi}\right) \cdot \left(\frac{d\pi}{dy}\right);$$

therefore, we have

$$\frac{d\left(\frac{dV}{dy}\right)}{dy} = \left(\frac{d^2 V}{dy^2}\right) = \left(\frac{dV}{dr}\right) \cdot \frac{d.\left(\frac{dr}{dy}\right)}{dy} + \left(\frac{dr}{dy}\right) \cdot \frac{d.\left(\frac{dV}{dr}\right)}{dy} + \left(\frac{dV}{d\theta}\right) \cdot \frac{d\left(\frac{d\theta}{dy}\right)}{dy} + \left(\frac{d\theta}{dy}\right) \cdot \frac{d\left(\frac{dV}{d\theta}\right)}{dy} + \left(\frac{dV}{d\pi}\right) \times \frac{d\left(\frac{d\pi}{dy}\right)}{dy} + \left(\frac{d\pi}{dy}\right) \cdot \frac{d\left(\frac{dV}{d\pi}\right)}{dy};$$

$$i.\,e. \left(\frac{d^2 V}{dy^2}\right) = \left\{\begin{array}{l} \left(\frac{dV}{dr}\right) \cdot \left(\frac{d^2 r}{dy^2}\right) + \left(\frac{dr}{dy}\right) \cdot \left[\left(\frac{d^2 V}{dr^2}\right) \cdot \left(\frac{dr}{dy}\right) + \right. \\ \qquad \left. \left(\frac{d^2 V}{dr.\,d\theta}\right) \cdot \left(\frac{d\theta}{dy}\right) + \left(\frac{d^2 V}{dr.\,d\pi}\right) \cdot \left(\frac{d\pi}{dy}\right)\right] + \\ \left(\frac{dV}{d\theta}\right) \cdot \left(\frac{d^2 \theta}{dy^2}\right) + \left(\frac{d\theta}{dy}\right) \cdot \left[\left(\frac{d^2 V}{d\theta.dr}\right) \cdot \left(\frac{dr}{dy}\right) + \right. \\ \qquad \left. \left(\frac{d^2 V}{d\theta^2}\right) \cdot \left(\frac{d\theta}{dy}\right) + \left(\frac{d^2 V}{d\theta.d\pi}\right) \cdot \left(\frac{d\pi}{dy}\right)\right] + \\ \left(\frac{dV}{d\pi}\right) \cdot \left(\frac{d^2 \pi}{dy^2}\right) + \left(\frac{d\pi}{dy}\right) \cdot \left[\left(\frac{d^2 V}{d\pi.dr}\right) \cdot \left(\frac{dr}{dy}\right) + \right. \\ \qquad \left. \left(\frac{d^2 V}{d\pi.d\theta}\right) \cdot \left(\frac{d\theta}{dy}\right) + \left(\frac{d\ V}{d\pi^2}\right) \cdot \left(\frac{d\pi}{dy}\right)\right]. \end{array}\right.$$

$$\text{III. } \left(\frac{dV}{dz}\right) = \left(\frac{dV}{dr}\right) \cdot \left(\frac{dr}{dz}\right) + \left(\frac{dV}{d\theta}\right) \cdot \left(\frac{d\theta}{dz}\right) + \left(\frac{dV}{d\pi}\right) \cdot \left(\frac{d\pi}{dz}\right).$$

Hence,

$$\frac{d\left(\frac{dV}{dz}\right)}{dz} = \left(\frac{d^2 V}{dz^2}\right) = \left(\frac{dV}{dr}\right) \cdot \frac{d\left(\frac{dV}{dz}\right)}{dz} + \left(\frac{dr}{dz}\right) \cdot \frac{d\left(\frac{dV}{dz}\right)}{dz} + \left(\frac{dV}{d\theta}\right) \cdot \frac{d.\left(\frac{d\theta}{dz}\right)}{dz} + \left(\frac{d\theta}{dz}\right) \cdot \frac{d\left(\frac{dV}{d\theta}\right)}{dz} +$$

(dV

$$\left(\frac{dV}{d\pi}\right) \cdot \frac{d\left(\frac{d\pi}{dz}\right)}{dz} + \left(\frac{d\pi}{dz}\right) \cdot \frac{d\left(\frac{dV}{d\pi}\right)}{dz}; \textit{ i. e.}$$

$$\left(\frac{d^2V}{dz^2}\right) = \left\{\begin{array}{l} \left(\frac{dV}{dr}\right) \cdot \left(\frac{d^2r}{dz^2}\right) + \left(\frac{dr}{dz}\right) \cdot \left[\left(\frac{d^2V}{dr^2}\right) \cdot \left(\frac{dr}{dz}\right) + \right. \\ \qquad \left. \left(\frac{d^2V}{dr.d\theta}\right) \cdot \left(\frac{d\theta}{dz}\right) + \left(\frac{d^2V}{dr.d\pi}\right) \cdot \left(\frac{d\pi}{dz}\right)\right] + \\ \left(\frac{dV}{d\theta}\right) \left(\frac{d^2\theta}{dz^2}\right) + \left(\frac{d\theta}{dz}\right) \cdot \left[\left(\frac{d^2V}{d\theta.dr}\right) \cdot \left(\frac{dr}{dz}\right) + \right. \\ \qquad \left. \left(\frac{d^2V}{d\theta^2}\right) \cdot \left(\frac{d\theta}{dz}\right) + \left(\frac{d^2V}{d\theta.d\pi}\right) \cdot \left(\frac{d\pi}{dz}\right)\right] + \\ \left(\frac{dV}{d\pi}\right) \cdot \left(\frac{d^2\pi}{dz^2}\right) + \left(\frac{d\pi}{dz}\right) \cdot \left[\left(\frac{d^2V}{d\pi.dr}\right) \cdot \left(\frac{dr}{dz}\right) + \right. \\ \qquad \left. \left(\frac{d^2V}{d\pi.d\theta}\right) \cdot \left(\frac{d\theta}{dz}\right) + \left(\frac{d^2V}{d\pi^2}\right) \cdot \left(\frac{d\pi}{dz}\right)\right]; \end{array}\right.$$

Hence, equation (A), or, $0 = \left(\frac{d^2V}{dx^2}\right) + \left(\frac{d^2V}{dy^2}\right) + \left(\frac{d^2V}{dz^2}\right)$, becomes

$$0 = \left\{\begin{array}{l} \left(\frac{dV}{dr}\right) \cdot \left[\left(\frac{d^2r}{dx^2}\right) + \left(\frac{d^2r}{dy^2}\right) + \left(\frac{d^2r}{dz^2}\right)\right] + \\ \left(\frac{dV}{d\theta}\right) \cdot \left[\left(\frac{d^2\theta}{dx^2}\right) + \left(\frac{d^2\theta}{dy^2}\right) + \left(\frac{d^2\theta}{dz^2}\right)\right] + \\ \left(\frac{dV}{d\pi}\right) \cdot \left[\ 0 \ + \left(\frac{d^2\pi}{dy^2}\right) + \left(\frac{d^2\pi}{dz^2}\right)\right] + \\ \left(\frac{d^2V}{dr^2}\right) \cdot \left[\left(\frac{dr^2}{dx^2}\right) + \left(\frac{dr^2}{dy^2}\right) + \left(\frac{dr^2}{dz^2}\right)\right] + \\ \left(\frac{d^2V}{d\theta^2}\right) \cdot \left[\left(\frac{d\theta^2}{dx^2}\right) + \left(\frac{d\theta^2}{dy^2}\right) + \left(\frac{d\theta^2}{dz^2}\right)\right] + \\ \left(\frac{d^2V}{d\pi^2}\right) \cdot \left[\ 0 \ + \left(\frac{d\pi}{dy^2}\right) + \left(\frac{d\pi^2}{dz^2}\right)\right] + \\ 2.\left(\frac{d^2V}{dr.d\theta}\right) \cdot \left[\left(\frac{d\theta}{dx}\right) \cdot \left(\frac{dr}{dx}\right) + \left(\frac{d\theta}{dy}\right) \cdot \left(\frac{dr}{dy}\right) + \left(\frac{d\theta}{dz}\right) \times \right. \\ \qquad \left. \left(\frac{dr}{dz}\right)\right] + \\ 2.\left(\frac{d^2V}{dr.d\pi}\right) \cdot \left[\ 0 \ + \left(\frac{d\pi}{dy}\right) \cdot \left(\frac{dr}{dy}\right) + \left(\frac{d\pi}{dz}\right) \cdot \left(\frac{dr}{dz}\right)\right] + \\ 2.\left(\frac{d^2V}{d\theta.d\pi}\right) \cdot \left[\ 0 \ + \left(\frac{d\theta}{dy}\right) \cdot \left(\frac{d\pi}{dy}\right) + \left(\frac{d\pi}{dz}\right) \cdot \left(\frac{d\theta}{dz}\right)\right]. \end{array}\right.$$

Hence, by substitution,

$0 =$

$$0 = \left\{ \begin{array}{l} \left(\frac{dV}{dr}\right) \cdot \left[\frac{\sin \theta^2}{r} + \frac{\sin^2 \pi + \cos^2 \pi . \cos^2 \theta}{r} + \frac{\cos^2 \pi + \cos^2 \theta . \sin^2 \pi}{r}\right]; \\ \quad \text{or, } \left(\frac{dV}{dr}\right) \cdot \frac{2}{r}; \\ + \left(\frac{dV}{d\theta}\right) \cdot \left[\frac{2 \sin \theta . \cos \theta}{r^2} + \frac{\cos \theta}{r^2 . \sin \theta} . (\sin^2 \pi - 2 \sin^2 \theta \times \right. \\ \quad \left. \cos^2 \pi) + \frac{\cos \theta}{r^2 \sin \theta} . (\cos^2 \pi - 2 \sin^2 \theta . \sin^2 \pi)\right]; \\ \quad \text{or, } \left(\frac{dV}{d\theta}\right) \cdot \frac{\cos \theta}{r^2 . \sin \theta}; \\ + \left(\frac{dV}{d\pi}\right) \cdot \left[\frac{2 \sin \pi . \cos \pi}{r^2 . \sin^2 \theta} - \frac{2 \sin \pi . \cos \pi}{r^2 . \sin^2 \theta}\right]; \text{ or, } \ 0; \\ + \left(\frac{d^2 V}{dr^2}\right) \cdot \left[\cos^2 \theta + \sin^2 \theta . \cos^2 \pi + \sin^2 \theta . \sin^2 \pi\right]; \\ \quad \text{or, } \left(\frac{d^2 V}{dr^2}\right); \\ + \left(\frac{d^2 V}{d\theta^2}\right) \cdot \left[\frac{\sin^2 \theta}{r^2} + \frac{\cos^2 \theta . \cos^2 \pi}{r^2} + \frac{\cos^2 \theta . \sin^2 \pi}{r^2}\right]; \text{ or, } \\ \quad \left(\frac{d^2 V}{d\theta^2}\right) \cdot \frac{1}{r^2}; \\ + \left(\frac{d^2 V}{d\pi^2}\right) \cdot \left[\frac{\sin^2 \pi}{r^2 . \sin^2 \theta} + \frac{\cos^2 \pi}{r^2 . \sin^2 \theta}\right]; \text{ or, } \left(\frac{d^2 V}{d\pi^2}\right) \cdot \frac{1}{r^2 . \sin^2 \theta}; \\ + 2\left(\frac{d^2 V}{dr . d\theta}\right) \cdot \left[\frac{-\sin \theta}{r} . \cos \theta + \frac{\cos \theta . \cos \pi}{r} . \sin \theta . \cos \pi + \right. \\ \quad \left. \frac{\cos \theta . \sin \pi}{r} . \sin \theta . \sin \pi\right]; \text{ or, } 0; \\ + 2\left(\frac{d^2 V}{dr . d\pi}\right) \cdot \left[\frac{-\sin \pi}{r . \sin \theta} . \sin \theta . \cos \pi + \frac{\cos \pi}{r . \sin \theta} . \sin \theta \times \right. \\ \quad \left. \sin \pi\right]; \text{ or, } 0; \\ + 2\left(\frac{d^2 V}{d\theta . d\pi}\right) \cdot \left[\frac{\cos \theta . \cos \pi}{r} . \frac{-\sin \pi}{r . \sin \theta} + \frac{\cos \pi}{r . \sin \theta} + \frac{\cos \theta . \sin \pi}{r}\right]; \\ \quad \text{or, } 0; \end{array} \right.$$

Consequently, equation (A), after multiplying by r^2, becomes

$$0 = 2\left(\frac{dV}{dr}\right) . r + \left(\frac{d^2 V}{dr^2}\right) . r^2 + \frac{\cos \theta}{\sin \theta} \cdot \left(\frac{dV}{d\theta}\right) + \left(\frac{d^2 V}{d\theta^2}\right) + \frac{\left(\frac{d^2 V}{d\pi^2}\right)}{\sin^2 \theta}$$

; which agrees with Laplace; since the first two terms are $= r . \left(\frac{d^2 . rV}{dr^2}\right)$: For $\left(\frac{d . rV}{dr}\right)$ is $= V + r . \left(\frac{dV}{dr}\right)$;

and, $\frac{d\left(\frac{d . rV}{dr}\right)}{dr}$ is $= \frac{d\left[V + r . \left(\frac{dV}{dr}\right)\right]}{dr} = \left(\frac{dV}{dr}\right) + \frac{d\left[r . \left(\frac{dV}{dr}\right)\right]}{dr}$

$$= \left(\frac{dV}{dr}\right) + \left(\frac{dV}{dr}\right) \cdot \frac{dr}{dr} + r . \frac{d\left(\frac{dV}{dr}\right)}{dr} = 2 . \left(\frac{dV}{dr}\right) + r . \left(\frac{d^2 V}{dr^2}\right).$$

III. *De-*

III. *Description of a Mercurial Voltaic Conductor. By* W. H. Pepys, *Esq. F.R.S.**

The advantages obtained by perfect contact in Voltaic conductors is well known to the experimentalist, particularly when the combinations or series of plates are but few. Hence the slightest oxidation or corrosion of the wires destroys more than half the effect.

Having with others noticed the complete contact which quicksilver gives, I had an apparatus so constructed as to unite this advantage with the facility of using the wires or conductors in almost all the modifications that are required in the valuable and interesting experiments of Sir H. Davy on the electrical laws of chemical decomposition.

This apparatus has also another claim to notice, from the operator not being so likely to receive the charge, when the combinations are extensive, the adjusting-sliders being non-conductors of electricity.

With this apparatus and a series of six troughs of ten four-inch plates, I have decompounded solutions of the neutral and several of the more solid salts, such as gypsum, chalk, and fluor spar; deflagrated charcoal, phosphorus, and the metals; and formed the alloys of sodium, potassium, and ammonium with mercury.

Reference to Plate I.

Transparent View of the Apparatus, showing the Inside Arrangements of the Box.

A and B. Two cells formed by a partition of glass at C. They are to be filled about a third with quicksilver.

D and E. The negative and positive conducting wires from the Voltaic battery entering the quicksilver in the cells.

F and G. Two tubes of glass filled with quicksilver, with platina wires cemented into their lower ends, attached to sliders in the top of the box, and moving freely in the cells of quicksilver beneath.

H and I. Two moveable platina wires entering the glass tubes in contact with the mercury. These wires are variously formed at the will of the operator: those shown in the apparatus are pointed at one end, and being slightly bent at the other may be adjusted to any heights in the quicksilver of the tubes.

K. Platina crayon-holder for receiving slips of charcoal or rolls of metallic leaf for deflagration.

L, M, and N. Series of one, two, and three vessels (in

* Communicated by Mr. Pepys.

stands

stands for their security) for holding solutions, &c. exposed to the Voltaic conductor. The communications where more than one is used are made by asbestos, &c.

O and P. Vases or cups turned in gypsum, chalk, or fluor spar, and filled with water or coloured solutions, for the purpose of exhibiting the decomposition of such bodies, as before mentioned.

The apparatus and its appendages were constructed under my direction by Mr. Bate, philosophical instrument-maker, Poultry, London.

IV. *On the Difference between the Hydro-carbonated Gases extricated from Mineral and Animal Substances respectively*.*

Messrs. Thenard and Dupuytren, within these two or three years, made an experiment which has thrown considerable light on the existence of miasmata. They agitated distilled water with hydro-carbonated gas extricated from mineral substances. This water, exposed to the air and allowed to stand, was not disturbed, and gradually got rid of its hydrogen gas without being corrupted. The same experiment made with hydro-carbonated gas coming from animal putrefaction presented another result. The water became turbid, it contained flakes of a substance truly animal, which was precipitated on being allowed to rest, and the liquid was putrefied. Thus, although the gas was the same to the eyes of the experimenter, the latter contained manifestly miasmata which gave rise to the flakes observed, and to the putrefaction of the water.

M. Moscati, an eminent Italian physician, has made similar and equally interesting experiments. Having observed that the cultivation of rice in the humid rice grounds of Tuscany was annually attended with epidemic diseases and adynamic fevers, he conceived the idea of ascertaining the nature of the vapours which rose from the ground where rice was cultivated: with this view he suspended at some distance from the ground hollow spheres filled with ice. The vapours were condensed on the spheres in the form of hoar frost. He collected this substance in flasks, in which it melted and at first presented a clear liquid. Speedily it was filled with small flakes, which when collected and ana-

* *Annales de Chimie*, tome lxxxii. p. 330.

lysed,

lysed, presented all the characters of an animal matter. The liquid in a short time putrefied. M. Moscati made the same experiment in an hospital, by suspending the glass spheres over several sick persons: it was attended with the same phænomena and the same results. These experiments ought to be repeated and followed up: they might be varied, multiplied, and compared, with a view to elucidate the theory of contagion which takes place without immediate contact. In this way we might also examine the alteration which miasmata undergo, when the nitric or muriatic fumigations are resorted to.

V. *Comparative Analysis of the Urine of various Animals.* *By M.* Vauquelin*.

The only kinds of urine which chemists have hitherto analysed in a satisfactory manner have been those of men, and some of the larger herbivorous animals.

The urines of carnivorous and wild animals have not as yet, so far as I know, been examined by any person.

Nevertheless, if we admit that comparative anatomy on the one hand has greatly contributed to the advancement of physiology, we shall perhaps also ascertain that comparative chemistry may, on the other hand, be made conducive to this science.

Already has the urine of birds furnished results sufficiently interesting and unexpected to induce chemists to prosecute their experiments among all classes of animals which furnish this liquid, that they may not in future entirely rely on analogy. With this view I have analysed the urine of the royal tiger, the lion, and the beaver; the results of which I subjoin, intending to extend my experiments to other animals.

The urines of the lion and tiger are perfectly similar: they have also some resemblance to that of man, but they differ in some essential points.

First difference: they are alkaline, even at the instant of being voided; the urine of a man in health is, on the contrary, always acid.

It is to the presence of the ammonia developed in these urines that we ought to ascribe the strong and disagreeable smell which they diffuse, even when in the act of issuing from the bladder of this class of animals.

* *Annales de Chimie*, tome lxxxii. p. 197.

Second difference: they do not contain any uric acid, nor any combination of this acid with the alkalis. At least, there was no sensible trace when the experiment was four times repeated.

The defect of uric acid in these urines was the more remarkable, as I used to ascribe its formation to animal food.

The third difference exhibited by the urine of the lion and the royal tiger from that of man, was the almost total absence of phosphate of lime.

This is what might be naturally expected, since this salt cannot be dissolved in water except by means of a superabundance of acid, and the urine in question is on the contrary alkaline.

It would nevertheless seem that the kidneys of these animals separate a certain quantity of this salt from the blood; for I found slight traces of it in these urines; and ammonia is formed in the bladder only, where probably it precipitates phosphate of lime: and this is without doubt the reason that the urine of these animals issues from the bladder almost always in a turbid state.

If according to this we ever find calculi in the bladders of these animals, they can be formed of phosphate of lime only, since this is the only insoluble substance they contain.

Fourth difference: the urines of the lion and the tiger contain but an infinitely small quantity of muriate of soda; whereas that of men generally exhibits a great deal.

We find in these urines a great quantity of urea very much disposed to crystallization, and in general a little coloured; phosphates of soda and ammonia, sulphate of potash, a mucous matter, and a trace of iron.

The above are the points in which the urines of the lion and royal tiger resemble that of man; but they differ, as has been shown, in a sufficient number of points to warrant us in forming a particular species. It is composed as follows:

1. Urea.
2. Animal mucus.
3. Phosphate of soda.
4. Phosphate of ammonia.
5. Muriate of ammonia.
6. A trace of phosphate of lime.
7. Sulphate of potash in a large quantity.
8. An atom of muriate of soda.

Urine of the Beaver.

A careful analysis several times repeated of the urine of the

the beaver, proved that it has a great resemblance to the urine of the common herbivorous animals.

In fact, we there find carbonate of lime kept in solution by a superabundance of carbonic acid: benzoic and acetic acids, urea, muriate of soda and sulphate of potash; and we meet with no uric acid in it, or phosphoric salts.

Nevertheless it differs in so far as it contains no muriate of ammonia, and as possessing a considerable quantity of carbonate and acetate of magnesia, which is not found, at least in a great quantity, in the urine of herbivorous animals.

I discovered the carbonate of magnesia in the following manner:

After having concentrated by a gentle heat a certain quantity of this urine, I decanted the liquor, and washed with distilled water the vessel to the sides of which the carbonate of lime adhered. I afterwards passed sulphuric acid into it diluted with water, which produced a frothy effervescence on account of a mucous matter which carries off with it the carbonate of lime.

Perceiving that the sulphuric acid had acquired a bitter taste from this combination, I dried and calcined the mixture, then I washed it with a little water, and I obtained by the evaporation of the latter, a salt which had all the properties of sulphate of magnesia.

Wishing to ascertain by another experiment, if there was muriate of ammonia in the urine of the beaver, as well as in that of other herbivorous animals, I put into a portion of this thickened liquor a piece of caustic potash; and as the odour of the ammonia was not perceived even with the aid of heat, I concluded that it did not contain any muriate of ammonia: but a phænomenon was exhibited which astonished me, and which excited a desire to examine the cause of it. The liquor went into a gelatinous-like mass: suspecting that this effect was produced by the precipitation of some earthy substance, I treated the whole of the thickened urine which I possessed with caustic potash; I filtered the liquor in order to obtain the matter in question; and after having washed and calcined it, I combined it with sulphuric acid diluted with water, and obtained sulphate of magnesia mixed with a little sulphate of lime.

Although I have announced that the urine of the beaver contains acetate of magnesia, yet I am not perfectly certain of it: in fact, it may be possible that during the evaporation, although effected with a gentle heat, a certain

quantity of acetic acid may be formed, and the latter may have acted on the carbonate of magnesia left in the liquor, on account of its solubility being greater than that of the carbonate of lime.

We generally ascertain by the colour, smell and taste, and above all by the property which the urine of the beaver possesses of staining alumed cloths, the kind of vegetables on which the animal feeds.

In the urine of the animal which I made the subject of my experiment, I distinguished evident marks of the colouring matter of willow bark, and its keeper confirmed the observation.

There seem to be cases, therefore, in which certain vegetable substances may pass through the digestive organs, and the circulation, without entirely losing the properties which distinguish them in their natural state.

I also found in the urine of the beaver a quantity of iron, which at first astonished me; but having reflected that it had been collected in a tinned iron vessel, and that it contained carbonic acid, I thought that the greater quantity of this metal proceeded from the vessel.

The urine of the beaver is therefore composed of,

1. Urea.
2. Animal mucus.
3. Benzoate of potash.
4. Carbonate of lime and magnesia.
5. Acetate of magnesia (doubtful).
6. Sulphate of potash.
7. Muriate of potash *or of soda.*
8. Vegetable colouring matter.
9. Lastly, a little iron.

VI. *Observations on the Measurement of three Degrees of the Meridian conducted in England by Lieut.-Colonel* William Mudge. *By Don* Joseph Rodriguez. *Communicated by* Joseph de Mendoza Rios, *Esq. F.R.S.**

The determination of the figure and magnitude of the earth has at all times excited the curiosity of mankind, and the history of the several attempts made by astronomers to solve this problem might be traced to the most remote antiquity. But the details of the methods pursued by the ancients on this subject being extremely vague, and their

* From the Philosophical Transactions for 1812, part ii.

results

results expressed in measures of which we do not know the relation to our own, in fact give us very little assistance in learning either the figure or dimensions of our globe.

It was not till the revival of science in Europe that the two great philosophers Huyghens and Newton first engaged in the consideration of this question, and reduced to the known laws of mechanics the principles on which the figure of the earth should be determined.

They demonstrated that the rotatory motion should occasion differences in the force of gravity in different latitudes, and consequently that parts of the earth in the neighbourhood of the equator should be more elevated than those near the poles.

The most simple hypothesis, which first presented itself to their imagination, was that which supposed the earth to be throughout composed of the same kind of matter, and its surface that of a spheroid generated by revolution round its axis. This hypothesis, adapted by Newton only as an approximation to the truth, is, in fact, perfectly consistent with the equilibrium to which particles in a state of paste, or of tardy fluidity, would arrive in a short time after their present motion was impressed; and the eccentricity derived from this hypothesis is at least not very remote from that which actually obtains in the present state of consistence and stability which the earth has since acquired.

But the homogeneity of the matter of which the earth consists, is at variance with all geological observations, which prove evidently that at least 5000 toises of the exterior crust is formed of an immense mass of heterogeneous matters varying in density from each other; and upon the supposition of a state of fluidity of the whole, it should follow that the strata should successively increase in density from the surface towards the centre, that the more dense would accordingly be subjected to less of centrifugal force, and consequently that the spheroidical form resulting from this cause would be less eccentric than would arise from a state of perfect homogeneity.

The most simple as well as the most effectual means of verifying the hypothesis respecting the figure of the earth, is to measure in the two hemispheres several arcs of its meridians in different latitudes, at some distance from each other. On this subject it must be allowed, that the Academy of Sciences at Paris set the example, in giving the original impulse to the undertaking, and not only commenced,

menced, but put in execution, those parts of the plan which were most difficult and most decisive.

The results of the first measurements made of different arcs on the meridian of different parts of the world, were found to be perfectly conformable to the expectations of Huyghens and of Newton, and also with experiments made on the vibration of the pendulum in different latitudes; and they left no doubt that the earth was in fact flattened at the poles; establishing thereby one point extremely interesting in natural philosophy.

These results, however, did not correspond with sufficient accuracy for ascertaining with precision the degree of eccentricity, or even the general dimensions of the earth, as might naturally be expected when we consider the necessary imperfection of the means then employed in these operations, and the great difficulties that are to be encountered.

For the purpose of making a nearer approximation to the true dimensions of the earth, and of verifying former measurements, it is necessary in some instances to repeat them, and also to make others in different situations, which may be expected to be improved in proportion to the progress that is made in the means of perfecting the several departments of science.

At the commencement of the French revolution, men of science took advantage of the general impulse which the human mind received in favour of every species of innovation, or change, and they proposed making a new measurement of an arc of the meridian in France, for the purpose of establishing a new system of weights and measures, which should be permanent, as being founded on the nature of things.

A commission, composed of some of the most distinguished members of the Academy of Sciences, was charged to form the plan of these operations, which were to serve as the basis of the new system. They invented new instruments, new methods, new formulæ, and in short almost the whole of this important undertaking consisted of something new in science.

Two celebrated astronomers, Delambre and Mechain, were engaged to perform the astronomical and geodetical observations, and these they continued as far as Barcelona in Spain. The details of their operations, observations, and calculations, were subsequently examined by a committee of men of science, many of whom were foreigners collected

collected at Paris, who confirmed their results, and, by the sanction of such an union of talents, gave such a degree of credit and authenticity to their conclusions as could scarcely be acquired by other means.

Since that time, in the year 1806, Messrs. Biot and Arago, members of the National Institute, were sent into Spain for the express purpose of carrying on the same course of operations still further southward, from Barcelona as far as Formentera, the southernmost of the Balearic islands. Fortunately this last undertaking, which forms a most satisfactory supplement to the former, was completed by the month of May 1808, at a period when political circumstances would not admit of any further operations being pursued, as a means of verifying the results, by measuring a base which should be independent of those formerly obtained in France.

In the year 1801, the Swedish Academy of Sciences, encouraged by the success of the operations conducted in France, sent also three of its members into Lapland, to verify their former measurement taken in 1736, by new methods, and by the use of new instruments similar to those which had recently been used in France, and of which the National Institute made a handsome present to the Swedish Academy. The results of this new undertaking, which terminated in 1803, were drawn up by M. Svanberg, and are highly interesting, by their exactness, by the perspicuity of the details, and even a certain degree of novelty given to the subject by the arrangement adopted by the learned author M. Svanberg.

These new measures were found to confirm, in a remarkable manner, the general results of those which had preceded, and gave very nearly the same proportion for the eccentricity and other dimensions of the globe, so that there would not have remained the smallest doubt respecting the figure of the earth being flattened at the poles, had there not been a fourth measurement performed in England at the same time as that undertaken in Lapland, the results of which were entirely the reverse. This measurement, which comprised an arc of 2° 50′, was undertaken by Lieut. Col. Mudge, Fellow of the Royal Society, with instruments of the most perfect construction that had ever yet been finished by any artist, contrived and executed for that express purpose by the celebrated Ramsden. The details of the observations and other operations of Lieut. Col. Mudge may be seen in the volume of the Philosophical Transactions for the year 1803; and one cannot but admire the beauty

and perfection of the instruments employed by that skilful observer, as well as the scrupulous care bestowed on every part of the service in which he was engaged. Bengal lights were employed on this occasion as objects at the several stations, and their position appears to have been determined with the utmost precision by the theodolite of Ramsden, which reduces all angles to the plane of the horizon, and with such a degree of correctness, that the error in the sum of the three angles of any triangle is scarcely, in any instance, found to exceed three seconds of a degree, and in general not more than a small fraction of a second.

Accordingly the geodetical observations were conducted with a degree of exactness which hardly can be exceeded; and even if we suppose for a moment, that the chains made use of in the measurement of the bases may not admit of equal precision with the rods of platina employed in France; nevertheless, the degree of care employed in their construction, in the mode of using them, and the pains taken to verify their measures, was such, that no error that can have occurred in the length of the base could make any perceptible difference in the sides of the series of triangles, of which the whole extent does not amount to so much as three degrees.

Nevertheless, the results deduced by the author, from this measure alone, would lead to the supposition that the earth, instead of being flattened at the poles, is in fact more elevated at that part than at the equator, or at least that its surface is not that of a regular solid. For the measures of different degrees on the meridian, as reduced by Lieut. Col. Mudge, increase progressively toward the equator.

The following table of the different measures of a degree in fathoms is given by the author in his memoir.

Latitude.	
52° 50′ 30″	60766
52 38 56	60769
52 28 6	60794
52 2 20	60820
51 51 4	60849
51 25 18	60864
51 13 18	60890
51 2 54	60884

The singularity of these results excites a suspicion of some incorrectness in the observations themselves, or in the method of calculating from them. The author has not informed us in his memoir, what were the formulæ which he

he employed in the computations of the meridian; but one sees, by the arrangement of his materials, that he made use of the method of the perpendiculars without regard to the convergence of the meridians; and although this method is not rigorously exact, it can make but a very few fathoms more in the total arc, and will have very little effect on the magnitude of each degree. It is therefore a more probable supposition, that, if any errors exist, they have occurred in the astronomical observations. But it is scarcely possible to determine the amount of the errors, or in what part of the arc they may have occurred, excepting by direct and rigorous computation of the geodetical measurement. I have therefore been obliged to have recourse to calculations, which I have conducted according to the method and formulæ invented and published by M. Delambre.

The means generally employed for finding the extent of a degree of the meridian, consists in dividing the length of the total arc in fathoms, by the number of degrees and parts of a degree deduced from observations of the stars: but if these observations are affected by any error, arising from unsteadiness of the instrument, from partial attractions, or from any other accidental causes, then the degrees of the meridian will be affected, without a possibility of discovering such an error in this mode of operating. It is consequently necessary, in such a case, to employ some other method, which may serve as a means of verifying the observations themselves, of detecting their errors, if there be any, or at least of showing their probable limits.

My object therefore is to communicate the result of calculations that I have made, from the data published by Lieut. Col. Mudge in the Philosophical Transactions; and I hope to make it appear, that the magnitude of a degree of the meridian, corresponding to the mean latitude of the arc measured by this skilful observer, corresponds very exactly with the results of those other measurements that have been above noticed.

In M. Delambre's method nothing is wanting but the spherical angles, that is to say, the horizontal angles observed, corrected for spherical error. Moreover, for our purpose, we have no occasion for the numerical value of the sides of the series of triangles, but only for their logarithms. Thus the logarithm of the base measured at Clifton, as an arc, gives us that of its sine in feet or in fathoms, so that by means of this latter logarithm, and the spherical angles of the series of triangles, we obtain at once, and as easily as

in

in plane trigonometry, the logarithms of the sines of all their sides in fathoms.

After this, it is extremely easy to convert them into logarithms of chords or of arcs, for the purpose of applying them to the computation of the arcs on the meridian or azimuths. I give the preference to taking the logarithms of the sides as arcs, because the computations become in that case much more simple and expeditious.

Near to Clifton, which is the northern extremity of the arc, in a situation elevated 35 feet above the level of the sea, a base was measured of 26342,7 feet in length, the chains being supposed at the temperature of 62° Fahrenheit, or $13\frac{1}{3}$° Reaumur.

For reducing this base to toises, we have the proportion of the English foot to that of France, as 4 : 4,263, so that if p be taken to express the fractional part of the French foot, corresponding to English measure, then log. $p =$ 9,97234,46587, and then log. of 26,342,7 = 4,42066,02860; and hence the log. of the base in toises will be found equal to 3,61485,36943, and the number of toises corresponding is 4119,5 taken at the same temperature, which corresponds to $16\frac{2}{3}$° of the centigrade thermometer.

This base we must consider as an arc of a circle, and it is easy to reduce it to the sine of the same arc, according to the method given in a note at the end of this memoir. The logarithm of the *sine* of the base in toises is found to be 3,61485,35800.

With this quantity as base, and by means of the spherical triangles given by Lieut. Col. Mudge in his paper, I have found the logarithmic sines in toises of all the sides of his series of triangles, and have subsequently reduced them to logarithmic arcs of the same, which enable me to complete the rest of the calculation. With these we may compute any portions of the meridian, or successive intervals of different stations expressed in toises, and in parts of the circle, or their respective azimuths, having regard always to the relative convergence of different meridians.

The author has made observations for determining the latitude of the two extremities of his arc, and has also determined the azimuths of the exterior sides in his series of triangles by means of the greatest elongation of the pole star.

In the calculations that I have made, I began at Clifton in Yorkshire, the nothern extremity of the arc, and for this purpose

purpose the following are the data furnished by Lieut. Col. Mudge.

Latitude of Clifton reduced to the centre of the station 53° 27′ 36,″62.

Azimuth of Gringley, seen from Clifton, and reckoned from the north toward the west, 256° 17′ 25″.

Azimuth of Heathersedge, seen from Clifton, and reckoned in the same direction, 118° 8′ 8″,81.

With these data, and the two tables of spherical triangles, and the logarithms of their sides expressed in arcs, the intervals between Clifton and the two stations Gringley and Heathersedge were found in toises and in seconds of a degree, as well as all the corrections to be made on the first azimuths increased by 180°, as azimuths of Clifton seen on the horizon at these latter places.

The same process was continued for the following stations in succession, all the way to Dunnose in the Isle of Wight, which is the southernmost extremity of the series.

In this manner we have the latitudes and azimuths of each station, by means of two or three preceding stations, and consequently we have a verification of all the calculations that have been before made by Lieut. Col. Mudge.

The results of my calculations are contained in the two following tables.

First Table of Distances in Toises and in Seconds of a Degree on the Meridian, comprised between the westerly Stations in the Series of Triangles.

Names of the Stations.	Arcs in Toises.	Arcs in Seconds.
Clifton	0,0	0,0
Heathersedge	6834,324	430,9928
Orpit	15818,489	997,5928
Castlering	19801,1934	1248,8226
Corley	14295,384	901,6207
Epwell	22327,008	1408,2543
Stow	9555,479	602,7284
Whitehorse	18799,645	1185,8656
Highclere	14990,567	945,6354
Dean Hill	16105,614	1016,0180
Dunnose	23529,886	1484,4531
Sum total	162057,5437	10221,9837

Second

Second Table of successive Intervals between the Eastern Stations.

Names of the Stations.	Arcs in Toises.	Arcs in Seconds.
Clifton	0,0	0,0
Gringley	2809,105	177,149
Sutton	10838,816	1061,931
Holland Hill	4681,190	295,2251
Bardon Hill	18092,261	1141,0462
Arbury Hill	27956,417	1763,2683
Brill	22374,106	1411,2769
Nuffield	14350,3834	905,2155
Bagshot	12137,933	765,6822
Hindhead	14449,2027	911,5140
Butser Hill	7853,644	495,4551
Dunnose	20514,036	1294,1974
Sum total	162057,0941	10221,9607

Now if we take the arithmetic mean of the sums contained in the two tables, we have for measures of the entire arc, comprised between the stations of Clifton and Dunnose, the following quantities 162057,32 toises, and 10221,972 seconds of a degree, or 2° 50′ 21″,972. By dividing the former of these by the second, we get the measure of a degree, corresponding to the mean latitude of the whole arc, equal to 57073,74 toises, or 60826,34 fathoms, at the temperature of $16\frac{2}{3}$° of the centigrade thermometer, the latitude being 52° 2′ 20″.

The station at Arbury Hill happens to be very nearly in the meridian of Clifton and Dunnose, and divides the interval between them into nearly equal parts. The measures of that part of the arc which lies between Arbury and Dunnose, is by the tables 91679,47 toises, and 9783″,34 seconds, or 1° 36′ 23″,34 of the common division of the circle. The mean latitude of the arc is 51° 25′ 21″. And the measure of one degree corresponding to it is 57068,41 toises.

In the same manner the measure of the arc comprised between Arbury Hill and the northern extremity at Clifton is 70377,85 toises, and 4438,63 seconds, or 1° 13′ 58″,63. Its mean latitude is 52° 50′ 32″. And we have for one degree of the meridian, corresponding to this latitude, 57080,70 toises.

Hence, if we divide the entire arc into two equal parts, we deduce the following values of a degree corresponding to the middle of the whole and of its parts.

Latitudes.

Latitudes.	
51° 25 20″	57068
52 2 20	57074
52 50 30	57081

These values are, as appears, perfectly in conformity with the theory, and with the results of other measures that have been taken in different parts of the northern hemisphere: but, in order to place that agreement in a more distinct point of view, I shall show how nearly these estimates agree with the elliptic hypothesis, by comparing them with those measures of a degree on which we can place the greatest reliance for exactness.

Now, if we compare the results of these calculations with those deduced by Lieut. Col. Mudge from his observations, we shall see the probable source of those *errors*, which it appears to me have led him to false conclusions. It has already been observed, that the station at Arbury Hill divides the whole arc into two parts nearly equal, and that it is also nearly in the meridian of the two extremities at Dunnose and Clifton. It was, in all probability, this circumstance which determined the author to observe the latitude of Arbury Hill, as he would then have two partial arcs independent of the whole and of each other.

For determining the angular extent of these arcs, Lieut. Col. Mudge observed the zenith distances of several stars on the meridian above the pole, by means of a large zenith sector constructed by Ramsden with the same pains that he had bestowed upon the theodolite. Lieut. Col. Mudge paid all possible attention, and took all such precautions as might naturally be expected from an observer of his experience and address. Nevertheless the results of his observations made on different stars, differ no less than four seconds from each other. But, by taking a mean of all, the dimensions of the three arcs reduced to the centre at each station are as follows:

Between Clifton and Dunnose	2° 50′ 23″,35
Clifton and Arbury	1 14 3 ,40
Arbury and Dunnose	1 36 19 ,95

The extent of the first arc, in linear measure, is 1036339½ feet English, and when this is reduced to toises, we have for the lengths of the three arcs from Lieut. Col. Mudge's measures,

From Clifton to Dunnose	162067,3
Clifton to Arbury	70380,2
Arbury to Dunnose	91687,1

These

These last values exceed those resulting from my computations, the first by 10 toises, the second by 2, the third by 8 toises; and these differences arise from the convergence of the meridians, which the author thought might safely be neglected, and in fact it does not make a difference that is perceptible in the value of a degree upon the meridian. For the difference of 8 toises, in the distance between Dunnose and Arbury, makes but 5 toises difference in the value of a degree upon that arc, and the difference of 10 in the whole distance from Dunnose to Clifton, makes $3\frac{1}{2}$ in the measure of each degree on that arc. So that, as far as this source of disagreement is concerned, the author's results and mine would not be found to differ materially from each other.

But, if we attend to the angular dimensions of the several arcs, as deduced from observation and from calculation, these will not be found to agree so nearly.

The following table will show the differences in each instance.

Clifton and Dunnose	2° 50′ 23″,35	observed
	2 50 21 ,97	calculated
Difference	+ 1 ,38	
Clifton and Arbury	1° 14′ 3″,40	observed
	1 13 58 ,63	calculated
Difference	+ 4 ,77	
Arbury and Dunnose	1° 36′ 19″,95	observed
	1 36 23 ,34	calculated
Difference	− 3 ,39	

These differences are really considerable, and are capable of producing important errors in the results dependent on them.

In the first place we see, that the southernmost arc between Dunnose and Arbury is smaller than it would appear by computation, by as much as 3″,4; and when this deficiency is combined with an excess of 8 toises in the linear dimensions of the same arc, it makes as much as 40 toises difference in the estimated length of a degree. The reverse of this occurs in the northern portion of the arc comprised between Clifton and Arbury Hill. This is larger than it ought to be by 4″,77, and hence the value of a degree on the meridian turns out too small by about 62 toises in its linear dimensions. Fortunately, however, the excess

excess of the total arc is extremely small, as it does not exceed 1″,38, so as to make but five or six toises difference in the length of a degree observed on the meridian, and corresponding to the mean latitude of the arc examined.

[To be continued.]

VII. *On the Differential Thermometer.*

To Mr. Tilloch.

Sir, The simplest mode of settling the point in dispute between Professor Leslie and Sir H. Davy, with regard to the invention of the differential thermometer, is to give exact representations of Mr. Leslie's instrument, of Van Helmont's, and of the figures Sir Humphry has given of them.

Fig. 1. (Plate II.) is the differential thermometer of Mr. Leslie copied from the figure he gives of it in his work on Heat. Fig. 2. is the instrument of Van Helmont copied from the edition of his works 1648, and of which the figure in the edition 1652 is the same. Fig. 3. is the representation Sir H. gives of Van Helmont's instrument; and fig. 4. is that which he gives of Mr. Leslie's. No one I suppose can look at these figures without perceiving that they are not correct representations either of the one or of the other, but that both are essentially altered. The simple question, and which one would imagine might be easily answered, is, Why were these alterations made?

Instead of explaining this, a correspondent A. B. in your journal for November has said, as affording a ground of justification of Sir Humphry, that in the figure of Van Helmont "there is no aperture delineated, and it might be conceived hermetically sealed." He adds, that in the representation Mr. Leslie gave of this instrument in your journal, he introduced a cork, and his figure differs from those in the two editions of Van Helmont, which are found in the library of the College of Edinburgh, and which are perfectly alike.

To judge of the validity of this defence, it is only necessary to read Van Helmont's description of the instrument. "A et D sunt duæ sphæræ repletæ aëre. A autem et superior est exterius undequaque clausa, D vero est globus inferior apertus in fine canalis F. Sunt autem A et D ex unico vitro connexa per canalem BCE." He afterwards adds, "Liquor BC non potest se movere per temperamentum

mentum ambientis in canali, nisi unus globorum sit apertus, alter vero clausus." And further: "Sine apertura in F liquor BC non fuisset motus e loco."

It thus appears that Van Helmont's thermometer is described by him as open, in the most explicit terms. But to prevent the speedy dissipation of the liquor, the aperture or canal F is partly closed by a stopper, as appears from some other sentences in the description. This is not very well represented in the engraving, which is only a rude outline, and advantage is taken of this by A. B. to say that "there is no aperture delineated, and it might be conceived to be hermetically sealed." It is true that, from a view of the figure alone, it might not be evident what is meant to be represented at F. But a reference to the description which is immediately adjoining, the figure itself being in the text, points out by the most explicit statement that it is "a canal open at its extremity." This defence then by A. B. is merely a subterfuge. And it does not after all justify Sir Humphry's representation; for, in the figure he gives, there is no part corresponding to the canal F, and the instrument is represented hermetically sealed, without the possibility of inferring that it might be open. Why was this alteration made? The effect of it is to render the thermometer of Van Helmont essentially the same with Mr. Leslie's. But it would be hard to say that this had been the intention, and we must therefore suppose that some other adequate cause can be assigned.

The remark by your correspondent, that Mr. Leslie has introduced a cork, is rather frivolous; for whether a cork or a stopper is put into the canal F is of no importance; the important point which Van Helmont is so careful to state is, that there shall be an opening there. Mr. Leslie says nothing of a cork; and the error in the delineation of the instrument, alluded to by A. B. is so trivial that it does not deserve notice.

With regard to the form which Van Helmont gives to the instrument, and from which he calls it "suum organum," it is merely turning up the common air thermometer at the bottom, and giving it a ball, so as to form a bason, in which the liquor when depressed in the stem might be contained; a variation introducing no essential difference into the instrument: and it may be remarked, that nothing was more common than to vary the form of the air thermometer, as may be seen by a reference to the works of that period. Fig. 5. for example is the form of one given by Sanctorio in his "Commentary on Avicenna."

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The syphon barometer was one of the first variations of the common barometer, it being more convenient to have the bason forming part of the tube than detached. Van Helmont's variation with regard to the air thermometer is just the same; its under ball is open, and it could not be otherwise indeed, from the purpose to which he describes it as applicable, that of showing the temperature of the surrounding medium. Hence he loads Heer with reproaches for his stupidity in supposing it to be closed. If Heer however is to be ranked as an idiot, as Van Helmont says, (Heer autem apud idiotas ostendebat,) for this mistake, it would be still worse if the same mistake were now made after the very ample exposure of it by Van Helmont himself. It is indeed impossible that it could be made by any one who had read any part of the passage in which the instrument is described. It would be worse than a mistake. The only possible defence for Sir Humphry is, that he had never read the description; and the same defence, whatever stretch of candour it may require, must be extended to A. B.

To the remark by your correspondent, that Mr. Leslie has not quoted the sentence from Van Helmont in which the principle of the differential thermometer is described, and to the insinuation conveyed in the additional observation, that he "shall not pretend to determine whether Mr. Leslie has not read this passage, or whether he has read it and has not thought proper to quote it," it is sufficient to reply, that Mr. Leslie, in complaining of the injustice that had been done him, confined himself to the subject of his complaint, which he stated as briefly as possible. He was not called on to quote this passage more than some others; but that he did not seek to conceal it, as A. B. would insinuate, is evident from this, that he admits all that can be inferred from it,—that the statement of Val Helmont "incidentally involved the principle of the differential thermometer." This is the precise statement of the fact, and it does not invalidate Mr. Leslie's claim to the invention. A train of reasoning involving the principle of an instrument, or stating incidentally that principle, is very different from the actual construction and application to use of the instrument itself. Had the thermometer of Van Helmont been such as Sir H. Davy has figured it; had it been true that both its balls are closed, and that it "exhibits the action of heated upon cold air," it would have been an anticipation of the differential thermometer; and Van Helmont, we may be certain, would not have given such an instru-

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ment without displaying its advantages, and pointing out some use to which it was to be applied. He does no such thing. He is merely led, in the course of his illustration of his own instrument, to introduce an observation; in which observation, we, now that we are acquainted with the differential thermometer, perceive its principle. But he thought of no such instrument; nor did the state of knowledge at that time furnish any object of experiment to which it could have been applied. Mr. Leslie's invention originated from different views; it arose from the necessity of obviating the inconveniences of the common air thermometer to admit of its being applied to a particular subject of experiment; and it is injustice to him to represent the observation of Van Helmont as an anticipation of his invention.

If Sir H. Davy had remarked, that in the course of a dispute on a form of the air thermometer Van Helmont had incidentally stated the principle of the differential thermometer, Mr. Leslie could not have complained. And had Sir H. been eager to make this known to the world, he might without impropriety have quoted the passage as it stood *. But why say that Van Helmont figured an instrument very similar to the differential thermometer, when he figured no such instrument? Why alter the figure of the instrument which Van Helmont does give? Why alter it in such a manner as to bring it to resemble Mr. Leslie's? And why alter Mr. Leslie's so as to bring it, on the other hand, to resemble more nearly the one said to be Van Helmont's? These are the true points of discussion; it is needless to wander from them; and if Sir Humphry, or his friends, give satisfactory answers with regard to them, he will then stand exculpated from the charges which at present lie against him.

Some of your readers may think that more attention has been bestowed on this subject than it deserves. Others probably will be of a different opinion. The praise due to invention is often its sole reward; and any unfair or invidious attempt to lessen it, ought both in justice to the individual aggrieved, and from regard to the interests of science, to meet with due reprobation. Where there is any appearance of this, therefore, it ought not to pass unno-

* Such is the apparent eagerness of Sir Humphry on this point, that he introduces twice, first in the introduction, and afterwards in the body of his work, the statement, that Van Helmont had given a sketch of an instrument similar to the differential thermometer.

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ticed, while at the same time it will afford satisfaction to every one to find that a satisfactory explanation can be given.

I am, sir,

Your most obedient servant,

Edinburgh, Jan. 9, 1813. C. D.

VIII. *Dissertation on the Paintings of the middle Age. and those called Gothic. Extracted from an unpublished Work on Painting, by M.* Paillot de Montabert *.

The finest models of antiquity will always attract the attention of every philosophical artist; but the distance which separates us from the schools of antiquity, the influence of the manners and arts of the moderns, render these studies extremely difficult, and limit such subjects to minds of a peculiar description. If great efforts are therefore required, in defect of these dispositions, before we can even comprehend the theory of the ancients; and if, besides all this, we must shake off our tastes and habitudes, and many of our doctrines,—it is beyond all doubt, that every thing which can contribute to facilitate this great study ought to be carefully inquired into, and nothing which can assist us to attain the first sources ought to be neglected.

It has been remarked, that several *chefs-d'œuvre* of ancient sculpture have regained in the present æra that estimation which the observations of many years had not succeeded in procuring for them. Hence may we not conclude, that the opinions of many observers remain still in suspense with respect to numerous valuable productions which we do not yet comprehend? And it is not astonishing, when so many ancient sculptures and paintings appear to us to be weak and without substance, that the works of more degraded ages are treated with contempt. Nevertheless, although we ought to dwell upon works of excellence only, we ought to despise such only as are of a vitiated or degraded taste, and we ought to cherish some regard for works which, however humble, are the valuable propagators of the soundest principles.

Thus, therefore, as the object of every person who exercises and professes the arts, is very different from that of the person for whose gratification these arts are cultivated; it is the duty of every honest artist to delineate the essential

* Millin's *Mag. Encyclopédique*, March 1812.

characters wherever he meets with them, and to refer to first principles on every occasion when secondary principles are displaced, and perverted from their true destination, by prejudices.

I shall consequently endeavour, in the present dissertation, to call the attention of my readers in the first place to the condition of the arts among those of the modern nations who have respected and followed the doctrines of their precursors. I shall point out at the same time the ingratitude of these nations for their first masters, and the fate of these same arts among those who have despised and neglected these doctrines. I shall afterwards proceed to the examination of the history and styles of painting of the middle age, and of those which are called Gothic. Finally, I shall conclude my dissertation by the analysis of certain qualities of these paintings, and their parallel with those of Raphael, and even of some paintings of the present day.

I do not pretend, in this essay, to represent as excellent the paintings of the middle age: this would not be serving the cause of the arts: but I shall endeavour to substitute for the affected disdain of some writers, the just degree of consideration which they merit.

If my zeal for the arts causes occasional repetitions, I hope that my motives will plead my justification.

Causes of the Respect or Neglect for ancient Monuments, among the Nations who have cultivated the Arts.

If we consider the progress of the arts among nations, and if we follow their successive transmissions, we must be surprised at the disdain which is gradually developed among nations recently civilized, and at the ungrateful national pride which banishes the remembrance of the first masters of their arts.

The ancient people of Egypt, who inculcated with so much vigilance a respect for their mysteries, their arts and sciences, announced themselves as the fathers of miracles, and designated their country as the legitimate cradle of the sciences: but if our imagination, excited by these presumptuous pretensions, goes back for a moment to anterior æras, and seizes the first ages of the world; if we endeavour to collect the fragments of human knowledge which escaped the great catastrophe of the Deluge, we shall find even beyond that period traditional dates, and still more ancient cultivators of the arts, and the pretensions of these *soi-disant* inventors will vanish. What will become also of the claims of the Egyptians, if we reflect upon their intercourse

tercourse by the medium of the Red Sea with Persia and India?

The Egyptians seem therefore to have been indebted for their arts to more ancient nations, and they profited by them to a certain degree: perhaps, if they had not studied so much to exalt themselves, they would have carried the arts thus acquired much further; and in spite of their climate, their manners, their religion, and many other causes which might be advanced to justify their neglect, they would have produced *chefs-d'œuvre*, if they had been better able to take advantage of the doctrines of the ancients.

If we pass on to Greece, we shall find that her inhabitants were indebted to Egypt for the rudiments of the arts. If their admirers are unwilling to admit this, it cannot be denied that they derived great advantage from their first masters, and among all the favours which Heaven bestowed upon them, that conferred by the Egyptians was the greatest. The Greeks seem to have been fired with the same ambition as the Egyptians. Sculpture and painting, according to some of their historians, originated among them. The historians cited, without regard to method, in Pliny's works, announced the origin, progress, and perfection of the arts in Greece. If we can believe them, nothing was borrowed; all was created, even to the first elements, and from the fabulous effort of Dibutades down to the miracles of Apelles every thing belongs to them. Sycione and Corinth in particular disputed the glory of having invented painting. Even the authors of the inventions are named: it was, according to them, Cleanthes of Corinth who invented the art of drawing (*pictura linearis*). Thelephanes of Sycione added the perfection of shading. Ardices of Corinth shared this merit with him. It was Cleophanes of the same city, as it is said, who invented monochromous paintings, or the art of filling up the various contours with one and the same colour. Dinias, Carmidas, and Eumarus, were also inventors. Cimon of Cleona was the first who traced the muscles and blood-vessels, despised the routine of profile drawings called *catagrapha*, and first described the folds in draperies. At this æra we begin to believe the written authorities; and when afterwards biographers make us acquainted with the Barlarchi, the Polignoti, and other subsequent painters, we participate in their admiration of the efforts and genius of the Greeks: but if the latter were surpassed by any other nation, they were dissatisfied, and accused Minerva of having bestowed the arts upon others in preference to her legitimate worshippers.

If it be true that the goddess did not give birth to the arts among them in a state of perfection, and armed at all points as she came herself from Jupiter's brain, it is nevertheless certain that she initiated them in the primitive mysteries of their predecessors, and kindly showed them the efforts of nations who had previously cultivated them.

The Egyptians had therefore naturally a first acquaintance with the arts, and devoted themselves to studies which the Greeks afterwards followed with improved success. The latter had for a salient point, data sufficiently determinate; and the models which they afterwards brought from Asia, and from Etruria, contributed to accelerate their progress.

We now reject entirely the opinion of all those writers who have incessantly repeated that the arts in Greece were in a barbarous state; and if there are still some persons who do not acknowledge the influence of the arts in Egypt over those of Greece, the superb works which have recently appeared in France will remove all doubts on the subject.

Let us now descend to the times of the Romans. These victorious soldiers at first despised the arts, which their increasing luxury nevertheless attracted towards Rome, and this capital of the world saw the æras revived of Pericles and Alexander: from being pupils of the Greeks, they became their rivals. But this very disdainful spirit, that vanity which regarded nothing as perfect which they had not produced themselves, induced them to prefer their national and composite taste to the pure and simple Attic graces: their manners, corrupted by the conquest of Asia, were visible in their arts: the pride of the Romans dictated the laws to sculpture and painting; and instead of the philosophers and learned men going to study at Athens, as the Athenians formerly studied at Memphis and Thebes, a fastidious taste triumphed over the simplicity of nature, and the style of the ancient schools began to disappear. From this moment the gods and heroes of Homer, figured by the arts, had no longer the same majesty; and when Pliny informs us that in his day there were still some artists equally expert with the ancients, we are inclined to believe that this opinion savoured of the influence of the age; an influence to which the wisest men are subject. But without seeking for its effects among the many celebrated men of that age, I shall merely quote the words of Quinctilian, the pupil of the Greeks: he informs us, that he was acquainted with nothing more majestic or magnificent than the robes and insignia of the triumphant generals of that period. The natural and affecting simplicity of the Greeks

Greeks was therefore gradually abandoned for the richness and magnificence of combinations; and the arts assumed a character of degradation. Nevertheless, while we must admire the imposing aspect of the works of the best days of Rome, under the Antonini, we must repeat that the Greek models of the age of Pericles always shine with new lustre, and that the study of the medals of Sicily alone, or of the bas-reliefs from Athens, will always delight the artist of genuine taste and talents.

If we now turn our attention to the middle ages, when the southern parts of Europe groaned under barbarism, we must also ascribe the new calamities which befel painting to pride and disdain. National vanity had no longer any control: at these unfortunate æras the learned were more employed in collecting the fragments of the arts than in adding to their perfection; and this respectful modesty, which was not without its good effects, since it recalled the ages of simplicity, paved the way for the glory of Da Vinci and Raphael. In those days antiquity was again honoured, and simple nature was once more loved and respected. The influence of the events of the time was the only obstacle: the sciences were not studied, it is true, but the minds of men were purer; they were less cultivated, but good sense prevailed. In a word, this state of the arts afforded extensive grounds for indulging in hope, and nothing to fear for their advancement. It was in those days of comparative languor that the consolations of painting softened the bitterness of individual and national sufferings; its mild and beneficent fruits were cultivated; and Religion, freed from the persecutions of the Heathens, employed it to record her triumphs. The temples, the receptacles of the dead, and the monasteries opened their sacred asylums, and the art of painting portable portraits was universally cultivated. From Constantinople to Rome, and from Rome to Siberia, we find representations of the saints, the apostles, and the divine mysteries. So many efforts, seconded by the protection of the ecclesiastical councils, produced some valuable specimens of art. The fragments of antiquity which had been collected, the study of the most ancient paintings and sculptures of the Christian æra, the examples drawn even from the monuments of Paganism which had been recently annihilated,—all contributed to keep alive among the painters of those days the sentiments of candour and the principles of true dignity.

What right then have we to throw ridicule upon these first expressions of ardent gratitude and religion? How absurd

would it be in any modern critic, who, while he proclaims Raphael to be the prince of painters, should refuse to acknowledge as useful, or valuable, the very models which have enlightened and directed the first steps of that great man,—models, which were derived from the inestimable sources of antiquity!

After having pointed out the lamentable effects of that vanity which made the precepts of the ancients be despised; after having shown that it is our duty, not to create a new art, but rather to recover that of the ancients; I shall proceed to the history of the painters of the middle age, and to the classification of their different styles, which has been hitherto neglected.

Histories of the Paintings of the middle Age.

Libraries furnish us with few or no documents of this description, at least with none of a date previous to the present æra.

The most important modern work is by M. Agincourt, who has devoted nearly twenty years of a residence in Italy to the subject. His history, which is not yet published, cannot afford all the advantages which I could have wished; but when at Rome, I have so often heard the author expatiate on the interest which artists must necessarily feel in the study of these monuments, that I am convinced of the value of his labours to the art of painting.

Almost at the same instant there appeared another writer fired with the same ardour. In a work entitled "*Considérations sur l'Etat de la Peinture en Italie dans les quatre Siècles qui ont précédés celui de Raphael,*" M. Artaud has presented the world with a most valuable collection of facts. His work contains an account of upwards of 150 pictures anterior to Perugino, and several of which are works of the twelfth century.

M. Denon, whose indefatigable zeal and enlightened taste are so well known, has recently enriched our public collections at Paris with pictures of the above period collected by himself. These pictures cannot fail to fix the attention of the learned.

Among other nations, and the English in particular, a similar taste begins to spread, and there can be little doubt that the efforts of the zealous propagators of the ancient arts above alluded to, will be followed up by more precise and more extensive chronologies of the science.

But it is now time to speak of the cause of the scarcity of these paintings, the number of which ought to be immense.

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I shall mention three causes of their destruction. Their most ancient enemies were the Iconoclasts, or breakers of images, whose ravages spread far and wide: the second cause was the cupidity of persons, who destroyed them for the sake of the ornaments in gold and precious stones which surrounded them: and the third cause of their destruction, of all others the most afflicting, was that jealousy of their plagiarists, who not only annihilated them, but endeavoured to propagate a contempt for images which they nevertheless did not cease to consult, to imitate, and to lay under daily contribution. Finally, such was the influence of these disdainful enemies, that under Pope Julius they tore down without remorse from the walls of the Vatican a fresco by Perugino, to make room for one which Raphael was to execute,—although the latter, equally commendable for a great and enlightened mind, nobly opposed it. We may conceive therefore that these paintings are very rare in Italy, and even throughout Europe.

Of the Styles and Schools of the Paintings of the middle Age.

It belongs to those only who are familiar with the æra, the various dates and styles, to furnish ideas worthy of the public attention on the above topics: but as I conceive that an artist may have views and comparisons which may escape the observation of the mere virtuoso or antiquary, I have undertaken the task of making some classifications.

I am aware that it is of very little consequence to artists to know precisely at what time, and in what places, certain parts of the art of painting were abandoned, resumed, and lost again: but they will confess, I trust, that there are indirect ways of advancing the arts, and that none can be blamed in particular; and they must also admit that, although these views may not be new to every person, they may nevertheless possess some interest, since they bring to light the causes of the progress as well as those of the decline of the grand art of painting.

By the term middle age, I mean the æra which embraces the first period of the decline of the empire under Constantine the Great, and which coming down in the first place to the ninth century, which terminated the persecutions against the Iconoclasts, embraces the period of the revival of the arts in the twelfth and thirteenth centuries, when the famous modern masters flourished.

Amid the obscurity which veils more or less the historical facts of these times, the progress of the art of painting may

may nevertheless be traced. It is true that we are ignorant of the causes which prolonged the intervals of sleep, or lethargy, of all the fine arts, and there will perhaps be a perpetual oblivion both of the names of the principal artists who continued to cultivate the arts at certain periods, and of the æras at which these artists flourished. But as the art of painting comprehends only a determinate number of constituent parts, it is not impossible to ascertain the various influences which invited to the study of these various parts artists of every age and country; and the analysis alone of these same parts ought to be sufficient with us for fixing the degree of interest, or of esteem, which we ought to bestow on these different productions: thus we shall be able to distinguish the various schools from which they emanated.

Before entering upon this analysis, it is proper to refer to the opinion of Winckelman, Mengs, Webbs, Milizia, and some other modern writers, who have recognised the merit of the productions of the old schools. The period at which these authors wrote did not permit them to publish, without restriction, all the new ideas, the strength and truth of which should gradually lessen the veneration improperly paid by certain didactic writers to the idols of former centuries: but the same thoughts animated better artists; our pictures, statues, and in short all our arts have felt the happy effects, and we may explain ourselves without constraint now that persons are disposed to receive the truth.

The art of painting as practised by the ancients, although depressed, despised, and persecuted, nevertheless preserved its essential character. If the paintings executed at Rome under the emperors were not to be compared to the *chefs-d'œuvre* of Parrhasius and Zeuxis, they did not exhibit a corrupted taste, or encourage pernicious doctrines. The most humble productions of these times are not beyond the rules of art; nor do they present those fantasies since authorised by irresolute minds, or those wild ideas which ignorance may at all times regard as an intrepid and novel species of enthusiasm. It was when painting was almost entirely neglected, and languished without notice, that the Tuscans, wishing to become more civilized, laid the arts under contribution: but from what point did those artists set out, who wished to unite their celebrity to that of their country? and what were the rules and maxims of those painters, who from that æra filled all Italy with their names and their new manner? Did they confine themselves to taking up the art where the ancients had left it? Did they

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endeavour to restore to it the perfection of which it had been so long deprived? and were these new artists sufficiently enlightened to recognise the indestructible solidity of the foundations of the art among the ancients, thinking that they could build only upon such bases, and forsaking their new ideas and deceitful independence? Who can answer all these questions? Such observers and artists alone as are destitute of prejudices: those only clearly perceive the barrier which insulated the arts of Florence from antiquity. All the genius, all the merits of the Bandinelli, &c. &c. are within this barrier. It is true that the art was partially revived; but it was no longer that of antiquity; *i. e.* it was no longer directed, guided, and characterized by those documents which are adapted to it at all times and seasons. In pictures, the drawing department gained in perspective correctness, and lost in *naïveté:* it gained in intrepidity and energy, and lost in truth and proportion. Anatomy became a study of ostentation: the magnificence of form was factitious; the artist appeared greater than the work, and celebrity was confounded with perfection. The composition was abandoned to the caprice of the artist, and was only kept within bounds, when the recollections of the order and symmetry of the ancients occurred by chance to determine their wavering resolutions. The essential characters of nature being once misunderstood, those of art were abandoned, and became a subject of disputation in the schools. The artists, freed from the yoke of ancient doctrines, prolonged for some time their doubts as to the essentials of painting and sculpture; and at length they attained that which was inevitable, *i. e.* the celebrity of the most eminent dragged the rest along with them. It was no longer the science of Phidias, Praxiteles, Protogenes, which was to be recovered: the fiery and imposing sketches of Michael Angelo were alone to be imitated: it was no longer from nature that ideas were to be drawn and renewed, but rather from the academies already famous, protected by princes, and become the glory of the country and of all Italy. The modesty of a small number of philosophical artists, and their praiseworthy attempts, scorned all exaggerations; but their pictures were nevertheless praised, because nature was exhibited in them: but vanity continuing its progress, all succeeding men of genius, one after another, worshipped the mysteries of these new schools of superstition. The efforts made to shake off this propensity for the natural tastes of each, absorbed the faculties, and there remained no longer any moral method by which

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to ascend to the philosophy of those Greeks who were an honour to the art. This state of things continued to the present day, and the conventions of the *mannerists* had all the force of law in the workshops. All the talents of an infinite number of painters, very justly admired, do not destroy their opinion. It was reserved to our æra, and our schools, to abandon and annihilate prejudices so long combated without success. Painting and sculpture, alarmed and ashamed at so many humiliating degradations, finally threw themselves into the arms of nature: artists again resumed the route which she pointed out.

[To be continued.]

IX. *On the Teeth of Fishes, and Shells found in the Vicinity of Reading.* *By* D. PLENDERLEATH, *M.D., Reading.*

To Mr. Tilloch.

SIR, IN the vicinity of Reading, within a circumference of three miles, there are four brick manufactories. The pits which have been dug for the purpose of procuring materials for the formation of bricks, afford an opportunity of examining the strata of earth in this vicinity; and of however small importance individual observations of themselves may be—still, in a collective point of view, they may add to a mass of evidence which may eventually lead to the most interesting inductions in the science of mineralogy.

The celebrated Cuvier, in his la e publication on Organic Remains in the vicinity of Paris, has by patient investigation and indefatigable industry thrown considerable light on this subject. Disclaiming all speculative opinions, he has continued by unwearied application to examine nature herself; and has described the appearances which presented themselves, in plain and perspicuous language, avoiding as much as possible those technical terms which may have a reference to former and probably erroneous systems; for the knowledge of the constitution of the earth is still in its infancy, and it is only to be forwarded by the means which he has pursued. In reference to this opinion, I transmit to you for insertion in the Philosophical Magazine, the following appearances from actual observation of the pits above described. The greatest in depth does not exceed eighty feet from the surface to the bottom; and as they all coincide pretty exactly in the nature of their stratification, I shall select that one which has been longest opened,

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and in which the greatest number of teeth and shells has been discovered, for description. The layers are all horizontal, and the section is vertical. The first layer, and which is the foundation, is chalk. In this formation no organic remains have been discovered; it contains nodules of flints, which are usually in beds and adhering to the chalk. Immediately above is the stratum of sand, containing the remains of marine animals. Its thickness varies from two feet and a half to less than a foot. The bed containing these remains is not entirely of sand, as some clay may be distinguished among it. The oysters are commonly entire; one valve being connected with another, although the adhesion has become so slight that they may without difficulty be separated, and the laminæ of each valve may likewise be very easily divided. On separating the valves, I found that the place which had been occupied by the oyster was filled by an unctuous earth exactly of the same shape with the oyster.

The external appearance of these shells is in some instances entire; but they generally have the appearance of those oyster shells which have been exposed to heat, and when placed in a fire they do not crepitate. The outer surfaces are rough; the inner smooth. They differ very much in size. The largest which I have observed is six inches in length and four in breadth. That they exist in very considerable quantity is proved from the circumstance of their having been discovered for upwards of a century, since which time the workmen have been in the daily custom of finding them. Dr. Brewer, in the Philosophical Transactions for 1700, mentions them without adverting to the teeth which are found in the same formation. These teeth are of a triangular shape, a little bent, of a dark-leaden colour, and having their surfaces polished. They are very small, the longest not much exceeding an inch in length, with the interior surface in some longitudinally sulcated. They are found in considerable quantities, but no vertebræ or any other remains of marine animals have been discovered along with them. The land in which they are imbedded is coarser and of a darker colour than that which is superincumbent, containing a little clay and gravel; which I remark, because, in order to obtain any useful knowledge on these subjects, it is necessary that the examination of these bodies should be connected with that of the strata in which they are found. The next stratum is a thin one of blue-coloured earth, above which is a layer of sand without any adhesion of extraneous substances, and extending

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to the thickness of seven feet. The next in succession is the formation of red clay, which is commonly the greatest in thickness of the formations. The last and uppermost is the alluvial earth, which does not exceed the thickness of two feet. In the three other pits, the difference consists in the relative thickness of the different strata. In the one which is most advanced in a northern direction, the shells are wanting; but teeth, similar to the others, are discovered.

I may likewise mention that a vertical section, made within the extent of a mile from this northernmost pit, affords a quite different appearance of the stratification, as immediately under a thin covering of alluvial earth is chalk, which presents a singular appearance, the layers of chalk and of flints being alternate through the whole depth, and observing great regularity of distance. The Thames divides these two pits.

Many parts in this island afford opportunities of describing differences in stratification; and when many such surveys are made in parts of the globe separated from each other, and these appearances accurately compared, there is no doubt but that a true theory of the mineral kingdom will be the result.

I am, sir,
Your obedient humble servant,
D. PLENDERLEATH, M. D.

Reading, Jan. 21, 1813.

X. *Notices respecting New Books.*

Interesting Discoveries and Researches on the Foot of the Horse. By BRACY CLARK, F.L.S. &c. In two Parts. Quarto.

AMONG other discoveries contained in this work are:—The inflexion of the hoof at the heels, towards its centre, forming an elastic bow, important in diminishing the resistance to the weight and efforts of the animal:—A remarkable *band* discovered, passing from the sides of the frog round the upper part of the hoof, serving to connect it strongly with the skin, and to close the line or joint where they meet.

The real cause of the *running thrush* is explained on principles before unknown.

A most important defect detected, in the principle itself, of modern shoeing, more injurious than the abuses complained of, particularly to growing feet, and the elastic feet of blood horses, demonstrated by a striking and very decisive

cisive experiment. The fallacy of the French doctrine of *Pressure of the Frog exposed*, and why this method has not succeeded in actual practice. The cause of *ring-bones* in horses feet explained. A more correct description of *founder*, and the *mitigated founder*. An unknown and very singular organization of the *internal frog* exhibited, of constrated layers of tendon, which appear to break the force of external concussion, by each layer receiving in succession its effects.—A new and highly beautiful structure of plates of bone, on the sides of the *natural coffin-bone*, imparting a degree of elasticity to it, and which structure, is gradually obliterated by the overpowering effects of the iron in shoeing, &c. To these is added,

An *highly important* Essay detailing the result of several expensive experiments, which show that expanding the contracted feet of horses, is not generally attended with benefit or advantage, and a discovery of the very unexpected cause of this. Also,

An Essay, *respecting the Shoes of the Ancients*, proving, that their shoeing was without nails, and that the present art was not in use till after the fifth century of the Christian æra: remarks on the shoe of King Childeric's horse, and of the first nailed shoe on record, &c.

The Philosophy of Arithmetic, (considered as a Branch of Mathematical Science,) and the Elements of Algebra: designed for the Use of Schools, and in Aid of private Instruction. By JOHN WALKER, *formerly Fellow of Dublin College.* 8vo. 216 pp. Dublin 1812.

We cannot withhold our praise from the author of this work, for his attempt to place arithmetic in that rank which it ought to hold as one of the two great branches of mathematics. If we admit with Mr. Locke, that "number is that which the mind makes use of, in measuring all things that by us are measurable," ARITHMETIC certainly ought, as the author contends, to take precedence of GEOMETRY, with which it has a more necessary connection than some are willing to allow.

In the first chapter, Mr. Walker, after noticing that we are indebted to the Arabs for our present method of numeral notation, remarks that "we may be impressed with a conviction of its ingenious simplicity, if we reflect on the endless varieties and indefinite magnitude of numbers; and then observe, that we are enabled, by the aid of only ten characters (the nine significant figures and the cypher) to designate any numbers whatsoever with the utmost facility and

and distinctness; and this, in a form which subjects them most conveniently to arithmetical computation. The important utility of the contrivance it may be sufficient for the present to illustrate by the following remark. Most children of a very young age can with ease multiply or divide the number 67,489 by the number 508. But let the same numbers be expressed by the Roman method of notation, which prevailed in Europe before the introduction of the Arabic, thus—$\overline{\text{lxvii}}$.cccclxxxix and dviii;—a man will be puzzled to perform either operation. The Greeks employed a numeral notation similar to the Roman: and it is truly wonderful how their mathematicians (even with the aid of some mechanical contrivances) surmounted the difficulties which they had to encounter in their arithmetical calculations; while we know that they were engaged in some of a very long and complicated nature.

"Yet when we examine the fundamental principle of the Arabic notation, it becomes a matter of surprise that the invention was not of earlier discovery: for it proceeds on a principle extremely simple, and one that must have been employed in all ages, whenever there was a practical occasion of counting any very large number. We may illustrate the principle by supposing that we had to count a great heap of guineas. It is plain that unless we employ some check on our numeration, we shall be very apt to lose our reckoning, and get astray as we advance. What then is the most obvious method of securing accuracy in our reckoning? Is it not to count by tens, or some fixed number beyond which we never shall proceed? Thus, when we have reckoned ten guineas, we may lay them aside in one parcel, and proceed to count another parcel of ten. But to prevent the number of these parcels from accumulating so as to lead us astray, whenever we have counted ten such parcels we may make them up into a rouleau, containing therefore ten times ten guineas, or one hundred: and whenever we have ten such rouleaus, we may combine them into one set, consisting of ten hundred, or a thousand, guineas: and so on. And by this simple contrivance it would never be necessary to reckon beyond the number ten. Now it is precisely upon this principle that we proceed in designating numbers by the Arabic notation. The several columns of figures, from the right band column, are the compartments in which we dispose the several combinations of ten. The first column on the right hand is the place for all odd units, below ten: the next to it on the left hand, or second column, is the place

place for all parcels of ten, below ten such parcels: the third column, for all parcels of a hundred (or ten times ten) below ten: the fourth, for all parcels of a thousand (or ten hundred) below ten: the fifth, for all parcels of ten thousand below ten: the sixth, for all parcels of a hundred thousand (or ten times ten thousand) below ten: the seventh, for all parcels of ten hundred thousand (or a million) below ten, &c.

"Thus by the help of the nine significant figures and the cypher we are able to designate all numbers however great; and this, while each of the figures (called the ten *digits*, from the Latin word signifying *a finger*) always retains the same numeral significancy. For example, in the two numbers 57 and 570, the character 5 denotes in each the number five, and the character 7 the number seven: but in the former the 5 standing in the second column designates five parcels of ten each, or fifty; but in the latter, where it stands in the third column, it designates five parcels of a hundred each, or five hundred: and in the former, the 7 standing in the right hand column designates seven units; but in the latter, standing in the second column, designates seven tens, or seventy. And thus we see that the cypher, though it denote that there is *no* number belonging to its column, yet must be written; in order to bring the significant figures into their proper places.

"To facilitate numeration, we commonly mark off by a comma every period of six figures, commencing from the right hand, and often semi-periods of three figures. And as the name of a *million* is given to ten hundred thousand, so ten hundred thousand millions are called a *billion*; the place of which therefore commences at the thirteenth column. In like manner the names of a *trillion*, *quadrillion*, &c. are given to ten hundred thousand billions, trillions, &c.

"But here it is to be observed, that the facility with which we can designate the highest numbers, and perform every arithmetical calculation on them, has occasioned an insensibility to the enormous magnitude of the numbers of which we speak. One billion is very easily mentioned, and easily designated by a unit followed by twelve cyphers: thus—1,000000,000000. A child also can multiply or divide that number. But perhaps the reader will be surprised at the statement that there is not one billion of seconds in thirty thousand years; though there be 60 seconds in every minute, 60 minutes in every hour, 24 hours in every day, and in a solar year 365 days 5 hours 48 minutes

and about 48 seconds. At that calculation, the precise number of seconds in 30,000 years is only 946707,840000; or above 50 thousand millions less than one billion. So that the number of seconds, which have passed since the creation of the world, is considerably less than the fifth part of one billion. In fact, it is only by some such considerations that we can form any conception of numbers so immense.

"From the view we have taken of the Arabic notation, it is plain that a cypher, wherever it occurs, increases tenfold the value of every figure standing on its left hand; but does not affect the value of the figures standing on its right hand. It appears also that the several columns may be conceived to be headed with their respective titles, as *parcels* of a thousand each, of a hundred, of tens, &c."

To the preceding extract, which may serve as a specimen of the author's style, it is only necessary to add, that one great merit of the work is, the lucid and full, yet perspicuous, explanation of elementary principles, which it exhibits, without departing from the rigidness of demonstration. In the detail the author has been happy in the adaptation of examples to the doctrines they are proposed to illustrate, and they are so contrived as to interest the juvenile mind in the attainment of the results.

To those who make themselves acquainted with the scientific principles of common arithmetic, the Elements of Algebra offer no serious difficulty. Of these elements the author has given such a view as may introduce the student into that field of science, and enable him to make further progress "by the aid of the larger works extant on the subject."

XI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Jan. 14. THE Right Hon. the President in the chair.—A paper detailing Experiments on Arsenic, Part II. by William Lambe, M.D. was read.

In this paper Dr. Lambe has continued his observations on arsenic. He has related the effects of potash, of ammonia, and of lime, upon this metal, each of which substances has furnished new observations.

By heating white oxide of arsenic with sub-carbonate of potash, the oxide is divided into two parts: one portion is acidified, the acid combining with the potash: another part

part is reduced, a quantity of regulus being always found in the neck of the retort. Dr. Lambe found that this compound both absorbed and gave out atmospheric air. When the heat was very low, a large portion of the air of the vessels disappeared; when a brisk heat was used, the air of the vessels had received an augmentation, consisting of a mixture of oxygen and azote.

The action of ammonia was examined by uniting the alkali with arsenic acid, and decomposing the salt by heat. To succeed in this process, it is necessary to mix the salt previously with iron filings, otherwise the glass of the retort is dissolved. The gas which came over was received in three successive portions. The first was not examined, as consisting principally of the air contained in the retort; the second portion appeared to be pure azote; the third and last portion proved to be principally a new and peculiar gas, similar to that described in Dr. Lambe's former paper: it yields, by being inflamed with oxygen gas, both carbonic and nitrous acids; and Dr. L. has called it therefore *nitro-carbonic oxide*.

The effect of lime has led to the most unexpected results. It appears that when white oxide of arsenic and lime are heated together, a portion of the arsenic disappears, and in its place water and carbonic acid are found; nor does there appear (as far at least as has been hitherto examined) any other product. In some cases a large quantity of carbonic acid was evolved, independent of the portion absorbed by the lime; in other cases the whole, or nearly so, was absorbed by the lime. From these facts Dr. Lambe concludes that oxide of arsenic is in this experiment decompounded, and that it is composed of the substances which form the common elements of animal and vegetable matter; namely, carbon, hydrogen, and oxygen.

Jan. 21. A letter to the President, from the American Col. Humphries, on a new species of Sheep was read. This peculiar breed, it appears, originated from some unknown cause (probably a species of disease) in the state of Massachussetts in 179:. It was produced by a ewe which fed on the banks of a river much frequented by otters, and the shortness of this animal's legs suggested the idea that it must have been occasioned either by the imagination of the mother, or more substantial communication. What contributed to sanction this wild conjecture was the disappearance of the otters (a very natural circumstance in the life of this nomade race) from that part of

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the river. The chief peculiarities of this otter-sheep, as vulgarly denominated, are very short, crooked legs; very relaxed and feeble joints; a slow walk, a sickly constitution, and a generally meagre body,—owing, probably, to the difficulty and pain it must undergo in procuring its food. The reason for propagating this apparently diseased race was the want of quickset fences, and the general adoption of low stone walls, which were unequal to prevent the common breed of sheep from trespassing on corn or meadow lands. Its body usually weighs about 45lbs.; its wool is rather long and fine, and when crossed with the Merino breed yielded a fine long silky staple, and weighing from 3 to 4 lbs. each fleece. It is difficult to fatten, but propagates its species like the common sheep. Sometimes, however, where the common ewe had twins by a ram of this kind, the female lamb was like the mother, and the male like the father, and the contrast between two such lambs sucking the same ewe was striking. The defects, it appears, of this breed overbalance its advantages, as it is becoming extinct, and it was with difficulty that Colonel Humphries could procure one to dissect, and send the skeleton to the Royal Society. If it could not leap ditches, it was equally incapable of being driven to market, and the duration of its life was consequently uncertain, and rather limited. A physician who dissected one at Boston, called it by a more characteristic appellation, *Agkon*, from ἀγκων, an elbow.

A paper by Sir Everard Home was read on the coagulating glands in the stomachs of some animals and fowls. This short paper contained an account of some experiments made by the late Mr. Hunter, to ascertain the coagulating power of the different parts of the stomach of calves and fowls; from which it appeared that the cardiac portion yields the strongest runnet, and that the gastric juice is the chief liquid that effects coagulation or becomes runnet.

PHILOSOPHICAL SOCIETY OF LONDON.

Mr. Wright's Lectures on the Passions—(Forming the second Course of his Elucidation of the Oratorical Character).

Mr. Wright has during the present and last months resumed his labours in this department of science before a numerous audience.

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In recapitulating the leading features of his theory (maintained in the course delivered last spring*) the lecturer concluded his reference by the following concentration of its principle.

"The muscles and nerves constituting the animal frame are fitted to act in unison with each other, and thus the organs of sound in all animals produce uniform expression: that the sound expressive of tranquillity answers to musical phænomena: and that the sounds expressive of all the modifications of feeling correspond, also, with musical phænomena, and all the varieties of concord and discord; and these are subservient to the complexity of passion, emotion and sentiment. Laughing and crying are the simple signs by which man expresses pleasure or pain; and as all his passions are modifications of *love* and *hatred*, it follows that these simple expressions of look must also be modified to correspond with the diversity. And further, by analogy, as the simple signs of desire and aversion may be produced by physical as well as mental causes, and by a mere effort of the will as easily as by either, it follows that all the other passions may be produced in the same way."

From the paramount influence of passion over reason in the mass of mankind, "the orator, (said Mr. W.) by a *judicious exertion* of that art which enables him to counterfeit its external signs, will secure the sympathy and admiration of his auditory;—persuade while he endeavours to convince, and soften the heart while he improves and heightens the energies of the mind." Proceeding to enforce in strong language the necessity of this sympathy, and descanting on the requisites essential to its promotion, Mr. W. slightly touched on points he had before discussed. "If the orator (he observed) would arrest the hearts of an audience in his favour, his *truth* or *belief* in what he proposes to them must not, for a moment, be disputed: and that, as the expression of voice and gesture is the outward attribute of this accurate disposition of mind, the want of earnestness in an orator demonstrates either hypocrisy or imbecility."

"As no person can be called an orator, unless he possess oratorical feeling, and is enabled to depict any modification of the mind at *will*,—an arrangement of the passions, enumerating their never-erring outward effects upon the human character, cannot fail of proving extremely in-

* Vide Phil. Mag. vol. xxxix. p. 225—233.

teresting to students. I shall however at present only observe, that *sympathy* in the breast of an auditor will operate in proportion to the accuracy of the outward signs exemplified in the voice, look, and gesture of the orator."

The *obstacles* that commonly prevent the excitement of this indispensable sympathy formed the next consideration of the lecturer, all of which he traced to two distinguished causes,—carelessness and affectation. "They may (said he) be construed into either impudence or self-conceit, or wickedness and imbecility of mind, and total disregard to the interests of others. The one, by an overacted sensibility, renders calamity and misfortune ridiculous; the other, by an opposite temperament, represents virtue and sincerity under many of the disadvantages of vice and falsehood." Men, indeed, naturally treat with disdain or ridicule all bare and inefficient attempts to excite their attention. Self-love too, often takes the alarm at the implied imputation of being easily biassed. All incongruous associations naturally excite displeasure; and it is the discrepancy between appearance and reality which forms the basis of contempt, whether allied to pity or abhorrence. This principle, as far as respects affectation, was very appositely maintained by a quotation from the discriminating pen of Fielding, which concluded with the following observation: "Great vices are the proper objects of our detestation, smaller faults of our pity; but affectation is the only true source of the ridiculous *."—Pursuing the subject, we noticed a remark, the truth of which every one must frequently have felt in his commerce with the world. "It is possible (says Mr. W.) to be very careless and extremely affected."

The lecturer then *enlarged* on the media by which the minds of an auditory are engaged and impressed, in language to the following effect: "The organs of vision and sound are the instruments of communication and oratory. First, the mind, through the medium of the visual organs, is sensible of the various alterations of the muscular forms of the face, of the attitudes of the body and the motions of its limbs: and as particular alterations of appearance are known to indicate distinct operations of mind, man is in some sort provided intuitively with sensible feelings of what passes in the breast of his friend or his enemy. 2dly, Through the auricular organ, the mind judges of sound, whether as to shortness or length, mo-

* See Preface to Joseph Andrews.

notone,

notone, elevation or depression; it judges also of suspensions and pauses. Now these several qualities may characterize the expression of a whole sentence, a word, a syllable, or a single letter. And again, these varieties, as far as sounds go, may be rendered correspondent to all the modifications of oratorical expression, the complexity of passion, emotion and sentiment: by the intervention of pauses and suspensions, by the adaptation of long and short syllables, of sharp and flat tones, and the greater or less inflexions of voice, the spirits may be either dilated with the sensations of joy, or depressed by those of sadness and melancholy. Having reduced the types of our feeling to motion and sound, it remains to distinguish their oratorical qualities. The first comprises looks and gestures; the latter, the tones and tunes in which words are delivered, and the rests, pauses and respirations necessarily intervening."

"According to the modern definitions of the words look and gesture, we understand an appropriate attitude of the body and cast of the eye to the nature and import of the sounds we are pronouncing; but, united, their genuine meanings appear to be (as guilt will frequently betray itself notwithstanding every attempt made to conceal it) a probable picture of what passes in the soul. We may also define look and gesture to be natural systems of expression, which by all nations and degrees of men are most readily understood." As an apt corollary to this last position, the learned lecturer turned to the animated page of Sterne, and enriched his discourse by the description of Corporal Trim's pathetic appeal to ordinary feeling on the news of master Bobby's death.

"This excellent picture exhibits to the student the 'plain lines' of Hogarth. If the corporal had characterized his action by 'curved lines,' he would have appeared affected, and have displayed but little eloquence.

We have a natural and rooted dislike to any kind of affectation, and to no species a greater, than to that which is seen in a person who pretends to mimicry, or to courtly gesture, without possessing the advantages and talents they require.

"The mind of the corporal seems to have been adequately attuned; he received the impression with the acuteness of sensibility, and expressed himself in all the energy of natural feeling. But if his mind had been otherwise framed, or if it had been under the influence of either of the unfriendly passions, his expression, though delivered in

the same words, would have been tinctured with the existing passion, and his oratory have suffered from the alloy, and so passed by unheeded. But if all the common and necessary movements for the purposes of life are performed by men in 'straight or plain lines,' all the polished feelings, all the brilliant energies of the soul, are represented by them in the graceful and ornamental movements of curve lines." The homely action of Corporal Trim would never suit "the unconstrained view" of Akenside "through mountains, plains, through empires black with shade."

Notwithstanding the celebrated dictum of Pope, that "those move easiest who have learnt to dance," Mr. Wright gives a decided preference to the use of the foil. Though we presume he will not, with Sir Joshua Reynolds, say that "all awkwardness comes from the dancing-master," yet he maintained that, though the limbs might attain suppleness in the school of Terpsichore, more grace and elegance, more ease combined with energy, are diffused over the frame from the practice of fencing. "The reason (he says) is obvious: all the intellectual functions are in action before an adversary in a fencing assault, and this is discoverable from the expression of the intellectual muscles.—In dancing, we have only to take notice of the vacant stare of the minuet-dancer, to convince us all is inanity, and 'not a breath of thought is seen to move.'"

In prefacing his observations on the pronunciation of some simple sounds of our tongue, the lecturer took occasion to express an elegant and respectful tribute to the character and attainments of Mr. Walker; for whose indefatigable industry and nice discernment he professed, notwithstanding his dissent in some fundamentals of the science, the highest estimation.

"Words (continued Mr. Wright) may be divided into simple, compound, and imitative; the first and second, connected together in discourse, belong peculiarly to the elements of elocution, the third to the philosophy of feeling. But as the three classes of words, taken individually, suffer from affected or negligent articulation and inflexion,—as the intended passion cannot easily arise by mincing or confining the vowel sounds, by accentuating the syllables contrary to the best usage and authority, injudiciously vocalizing or aspirating the consonants; a few remarks on some particular sounds, and on the disputed modes of pronouncing certain words, cannot be considered irrelative to my present object.

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"It would be difficult to decide which is the greater enemy to language, ignorance, or affectation usurping the power of shortening, lengthening, adding syllables, altering accents, and contracting the sounds of vowels. Ignorance would persuade us that the sound of our first letter is confined to the broad *ah*; while affectation, mincing out *aye* upon all occasions, would deprive us of an easy use of the under jaw. And so strictly do they adhere to this perversion, that when the letter precedes the consonant *r*, as in *march*, and consequently rendered thereby broad, hesitation is not made in pronouncing it almost *match*. But these enemies to pronunciation would feel themselves surprised, were they informed that the letter is liable to six modifications;—*fate*, *fat*, *far*, *fall*, *fare*, and the *unaccentuated* sound when it stands for an article, as, *a* paper; or in the word *par*ticular, &c. It is the manner in which the second sound is pronounced in certain words, which marks, as far as this letter is concerned, the affectation or vulgarity of the speaker. Many provincial dialects do not admit the second sound, and consequently pronounce words of three letters, where the vowel is placed between two consonants, as *bad*, *can*, *hat*,—as *bard* (without the roughness of the *r*) or *bod*, &c.—But as we ought not to be supposed able to discover the birth-place of a gentleman by his pronunciation, his mode of delivery should be accommodated to the best usage. Although *cast*, *pass*, *plant*, &c. are precisely of the analogy of *bad*, *can*, *hat*, &c. yet adhering too rigidly to the correctness of analogy is the symbol of affectation: on the other hand, pronouncing them broadly, as if modified by *r* without its roughness, (as *parss*, &c.) is a symbol of vulgarity. Now, as we may suppose every public assembly to be composed of persons whose ears have been habituated to particular modes, the speaker's first care should be to utter the vowel so that, if possible, he may not offend the ear of either party. It should then be his aim to adopt a middle articulation, and this can easily be accomplished by dwelling on the second vowel's sound, viz. *pa-ass*, *pla ant*, &c.; or by contracting horizontally, a little, the mouth from the third sound. If this be admitted as useful in oratory, then there are *seven* sounds of the first letter. Sheridan has given three sounds, Walker four, and Perry six."

"No doubt can arise in the mind of a gentleman how words should be pronounced before an accomplished assembly. Custom, analogy, etymology, and contrast, should be equally regarded."

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In commencing his sixth lecture, Mr. Wright particularised the most prominent defects and difficulties of speech which strike us in others, or affect ourselves. He seemed decidedly of opinion, that none of these arise (with certain individual exceptions) from natural malconformation, but are the result of the bad example which persons may have had in their early childhood. "Children receive the first impressions of language by imitation, and are sure to copy articulate defects of mothers, nurses, and any who may be suffered to prattle their 'soft nonsense' to them. I repeat, (he continued) that the greater part of solecisms and impediments originate in the indolence, or ignorance, of persons concerned in the management of children."

Precise directions for the obviating these faults of articulation followed, of somewhat too diffuse a nature for our recollection; but from the attention the learned gentleman appears to have devoted to the physiological branch of his profession, we readily confide in their efficacy. After citing the well-known and illustrious example of Demosthenes, whose ardent and unwearied genius surmounted every impediment of nature or habit, Mr. Wright delivered a strong exhortation to the student to persevere in his arduous task.

Having dismissed the topic of impediments in articulation, the lecturer proceeded to give an important view of the necessity of a due adjustment of accentuation, which he pronounced inseparable from the art of persuasion.—"Accentuation has been defined by grammarians to bear the same relation to words as emphases do to sentences; but, as there are many words in sentences unaccentuated, and whole sentences unmarked by emphases, our grammatical definition of accentuation is therefore imperfect—'Exercise and temperance strengthen the constitution.' 'And' and 'the' are words without accents; and as there is no opposition in the sentence, (emphasis suggesting the idea of contradistinction,) neither of the words is emphatic. Now if we call '|Exercise|' a simple word, '|and temperance|' a rhetorical word, '|strengthen|' another simple word, and '|the constitution|' another rhetorical word, we perceive the propriety of calling accentuation a sudden upward or downward percussion of the voice, distinguishable not in all words, but in those classes above denominated simple and rhetorical. The term simple seems properly to apply to such words as are pronounced with established accentuation, while the latter suitably adheres to a certain principle of combination."

"The regulation of accent and quantity as applied to

words

words, independently of each other, is determined by custom and authority; but the compass and accentuation of words connected or joined together, are discovered by the natural powers of sensation, which reason corrects, improves, and methodizes.

"The study of rhetorical accentuation would assist in protecting the student against the appearance of coldness and intellectual imbecility; of being 'uniformly slow and regularly dull.'"

Concluding his observations on accent, Mr. W. remarked "that rhetorical accentuation is of the greatest consequence to the expression of passion: a very short time devoted to the acquirement of its theory, would render the avenues of sympathy more attainable than whole years of desultory practice without it. Its judicious exercise would assist, from time to time, in giving new stimulus to the listening powers of an auditory."

His next consideration was on "certain individual articulations, which are so constructed as to be sufficient of themselves to excite peculiar sensations.—Whenever we are under the immediate influence of passion, we naturally make use of such words as seem best calculated to imitate our feelings; and these words, without any reflection, are spoken with more expression and significancy than the rest: by dwelling plaintively upon their syllables, if the passion be grief—by gliding slowly and monotonously over them, if the passion be melancholy—and hurrying precipitately over them, if it be anger."

In some striking poetical illustrations then adduced by the lecturer, a single word, significantly echoing the sense, seemed, by a peculiar emphasis, almost to outrapture the meanings conveyed by the rest of the language. "It is the appropriate use of such words which gives to every passion characteristic variety. At the instant of pronouncing them, the whole soul seems to be in action, and the tones and tunes of voice, echoing its feelings, immediately check the common current of uniform cadence.

"In pronouncing words peculiarly imitative of sound and motion, the voice may enter into the full spirit of the imitation. Yet must the admonition of our immortal bard be duly considered—'If this be over done,' or 'come tardy off,' while it makes the unskilful laugh, it cannot but make the judicious grieve."

Our limits will not admit the notes we made on the remainder of this interesting discourse: we defer them, therefore, together with those we were enabled to collect on the seventh, eighth, and ninth lectures, till our next number.

EDINBURGH INSTITUTE.

A general meeting of the members of this institute was held in Mary's Chapel, on Tuesday the 22d of December, for the purpose of receiving communications on subjects connected with science, literature, and the arts. Dr. Millar in the chair.

Among other communications the following were received:

1. Account of a fact in meteorology, lately discovered by Mr. John Hutton. In certain states of the atmosphere a succession of small clouds appear over the summit of Arthur's Seat. Each of these clouds forms on the windward side of the hill, apparently about one hundred feet above the level of the summit, a line drawn perpendicularly from the centre of the summit forming an angle of about 80 degrees, with a line drawn from the same point to the place where the cloud begins to form on the windward side and an angle of about 60 degrees with a line drawn from that point to the place where the cloud disappears on the leeward side. The cloud passes right over the summit. After an interval of two or three minutes another is formed and disappears in the same way, and this continues. Mr. Hutton first observed this phænomenon in the end of July last, about ten o'clock in the evening, the wind blowing moderately from W. by S. Barometer 30, 11. He has observed it since in August and September, at different times of the day, and from different positions.

2. Account of a portable printing press, invented by Mr. John Ruthven, Edinburgh. In this contrivance the pressure is produced by a wheel and pinion acting on the end of a small lever. It has apartments for holding ink, balls, and every other article necessary, and prints off a form not exceeding the size of a duodecimo page with the greatest correctness and celerity. The press exhibited was about 21 inches long, 6 broad, and 10 high, weighing about 22lbs. including the page of types, chase, balls, ink, &c. and was worked with great ease. An extract from the minutes of the night's proceedings was printed off by it in the presence of the meeting, and distributed among the members.

3. An account of an improved syphon, by Mr. Archibald Kerr, mathematical instrument-maker. This instrument consists of a syphon with a stopcock; and a pump-barrel with a piston, valve, &c. The bottom of the barrel communicates with the inside, immediately above the stopcock, at the end of the lower leg, for the purpose of extracting

tracting the air and filling the syphon. The syphon is filled in an instant by one or two strokes of the pump with the hand, and the communication between the pump and the syphon can be cut off at pleasure by a stop-cock. The principle is applicable to all sizes of syphons, and almost every kind of liquor may be drawn off with the utmost facility. Mr. Kerr has already made many syphons on this plan, and they are found to save considerably both liquor and time. When constructed in this way, the difficulties attending the use of the common syphon are completely removed, and the instrument is rendered so perfect that it will probably be found incapable of any further improvement. A small one was exhibited and worked in presence of the meeting.

4. Account of another improved syphon by Mr. John Hutton. This syphon is extremely simple, and has been used by Mr. Hutton with much advantage in his chemical manufactory. It has a stopper at the extremity of the longer leg, and a valve opening inwards at the extremity of the other. It is filled in the usual way by inverting it, and pouring in the liquor at one end. After this, the stop-cock being shut, the syphon is placed in its proper position, with the end of the short leg immersed in the liquor. The stop-cock is then opened, and the liquor, forcing up the valve at the short end, flows out. When the quantity required is drawn off, the stop-cock is shut, the valve at the other end falls down, and the syphon remaining full can be laid aside; and when it is to be used again, nothing more is necessary than to put it into the liquor and turn the stop-cock. A syphon of this description, which Mr. Hutton has employed for some time, was exhibited and used in presence of the meeting.

At the close of the proceedings the President observed, that the regulations direct meetings of this kind to be held occasionally in the course of each session; and that, if conducted as the meeting had been that night, they would be productive of the greatest advantage in bringing into notice many useful inventions, and giving publicity to improvements, by which society at large might be benefited; and he recommended to those (strangers as well as members) who might have it in their power to make such communications, to bring them forward at future meetings.

KIRWANIAN SOCIETY OF DUBLIN.

Jan. 13th.—The receipt of several specimens was reported

ported by the secretary: among them were the following, which have now for the first time been noticed in the respective places: "Arsenical pyrites in quartz, found by Dr. Ogilby at Hoath, county Dublin—Gray ore of manganese with white lead ore from the Scalp, county Wicklow—Specular iron ore found by Dr. Ogilby in rocks of the trap formation, county Antrim."

A paper "On Light" was read by Cornelius Keogh, esq. proposing to the Society the trial of certain experiments intended to confirm the analogy between light and sound.

Andrew Carmichael, esq. read part of a paper of considerable length, in which he adduced many reasons in support of a new hypothesis of the nature of electricity, viz. That the sun is the source of that principle, as well as of light and heat; and that the deoxygenating ray is the base of the two electric fluids.—The facts he has collected tend to demonstrate, that, in combination with the colorific ray, it forms positive electricity; and, in combination with the calorific ray, negative electricity.—A more copious abstract of the theory will be given, after Mr. Carmichael has read the remainder of his paper.

IMPERIAL INSTITUTE OF FRANCE.

[Continued from vol. xl. p. 468.]

It does not become us to hazard an opinion, when botanists so eminent are at issue; but their discussion has at all events procured this incontestable advantage to science, namely, that each of them, endeavouring to support his opinion upon facts, has discovered and explained the internal structure of the seed, and the mode of germination of many plants which had been scantily or badly observed in this respect. As a general thesis, however, we shall venture to say, that we never can be sure of the constancy of a character, so long as its importance is not demonstrated by the kind of influence which it exercises; for every thing which rests only on simple empirical observations, however numerous they may be, may be overturned by a single observation of a contrary tendency. Now the influence of the number and of the various forms of parts in vegetables, is still too little known to entitle us to hope, for a long time, to give to botanical characters that degree of rational certainty which those of zoölogy have obtained.

We

We ought also to observe, that the detailed description of the family of the hydrocharideæ, which M. Richard has given in the course of this discussion, has a merit independent of the object in dispute; namely, that of determining more precisely the genera of which this family is composed, and the number of which M. Richard has raised to ten, because he has added five new ones to those with which we were already acquainted.

M. Lechenault de la Tour, one of the naturalists who sailed with Captain Baudin, has given us some details upon the trees with the juice of which the natives of Java, Borneo, and Macassar poison their arrows, and which, by the name of the *upas*, has made so much noise. Of these poisons there are two kinds, the *upas anthiara* and the *upas thieuté*. Both kill in a few minutes by the slightest puncture, but the latter is most violent: it is extracted from the root of a kind of strychnos, or nux vomica, which creeps up the branches of the tallest trees. The experiments made by Messrs. Delille and Majendie prove that it acts upon the spinal marrow, and causes tetanus and asphixia. The former oozes out from a large tree, which M. Lechenault calls *anthiara toxicaria*, and which belongs to the family of the nettles. Those who are wounded with an instrument, poisoned in this manner, evacuate copiously green and frothy matter, and die in violent convulsions. The natives eat the flesh of animals which die from similar effects, after merely cutting out the wounded part.

M. Decandolle, the professor of botany at Montpelier, has promised to publish the new or little known plants of the excellent garden of which he has the charge, giving occasionally observations upon the genera to which these plants belong; and he has presented to the Class specimens which promise favourably of his future labours. The hundred plates which this work will contain are already designed.

Our associate M. de Beauvois still continues his Flora of Opara and Benin, of which he has this year published the 12th and 13th numbers. He announces for the 14th a new division of the grasses, founded on the union or separation of the sexes, and on the composition of the flower and the number of its envelopes.

Physic and Chemistry.

Since the days of Black, it has been known that bodies are not vaporized without absorbing a great quantity of heat, and that every evaporation cools the body from which it emanates,

emanates, the more it is accelerated: on the other hand, we know that atmospherical pressure retards evaporation, and that this change of state takes place in vacuo the more speedily, the more perfect the vacuum is.

Mr. Leslie, fellow of the Royal Society of London, has thought to increase still more this effect, by placing under the recipient of the air pump bodies which are greedy of moisture, and which, seizing the vapour as fast as it is formed, multiplies indefinitely its production; and he obtained in this way a cold so rapid and violent, that water froze in a few minutes. This is a method of always having ice at command, almost without any other expense than the fire necessary for once more drying the body greedy of humidity which had been employed.

Highly concentrated vitriolic acid and the muriate of lime are the most convenient absorbents for this purpose.

Two young chemists, Messrs. Clement and Desormes, have been occupied with determining the limits of this process, and the degree of œconomy to which it can be carried; both from a calculation of the quantity of caloric contained in the vapour of the water, and from the quantity of charcoal necessary to produce a given quantity of vapour: they ascertained that it requires but little more than one part of charcoal to restore to its pristine state the absorbent which served to freeze 100 parts of water. Thus 100 pounds of ice will only require a pound and a few ounces of charcoal.

We may increase the effect, by preventing any caloric from getting access externally; and it is sufficient for this purpose to render the recipient less of a conductor of heat, by making it for instance of two plates of polished metal separated by a stratum of air.

We derive also from this acceleration of the evaporation in vacuo, increased by the presence of absorbents, a more evident advantage, when it is required merely to dry humid substances, because we then avoid making them undergo the action of the fire, which always alters them more or less.

Our associate the late M. Montgolfier had already conceived the idea of drying completely the sap of plants, and particularly the juice of raisins, by the pneumatic pump; and he ascertained that, by diluting the latter juice in water, after it had been dried, it might still be fermented, and good wine obtained from it; but it cost him too much labour.

It is nevertheless necessary to prevent their juices from freezing, an inconvenience which would not be less troublesome than that which results from the effects of fire.—

Messrs.

Messrs. Clement and Desormes have invented a very simple method of avoiding it. They surround the vessel which contains the juice to be evaporated with the absorbing matter; and in this way the caloric which is liberated from the vapour at the moment of absorption returns to the juice which we are evaporating, and this circulation furnishes what the new vapour requires.

We may employ this process with considerable œconomy, if we begin by reducing the juice to the state of syrup by means of a ventilator, which is also the invention of Montgolfier, and which Messrs. Clement and Desormes have described in the Annales de Chimie for October 1810. The air pump is applied only when the ventilator no longer produces any effect.

It is easy to see how useful for domestic purposes, and particularly for the navy and army, is this new art of preserving entire alimentary substances, by diminishing their weight, and transporting in a small compass to great distances the fermentable matter which ought to furnish wine and alcohol.

The same chemists purpose to apply the evaporation in vacuo to the drying of gunpowder, which will be less dangerous than the common method in which fire is employed.

They have also devoted their attention to the common process of evaporation by means of fire, and have discovered a method of doubling the effects of a given quantity of combustibles over a liquid, *e. g.* a saline solution. It is only necessary to collect the vapour of a first portion of the liquid, and to force it to pass through a second portion. This vapour, when very much heated, gives off a great portion of its caloric to the new liquid which it passes through.

But of all the arts, that which has reaped the most astonishing advantages from modern discoveries upon heat and vaporization, has been that of the distiller of spirits: the process which we are about to describe, is merely an imitation of those which have been attended with a small portion only of these advantages.

This revolution, which already exercises a great influence over the prosperity of our Southern departments, originated with the late Edward Adam, a distiller of Montpelier.

The basis of his process consists in heating a great part of the wash to be distilled by the vapour of spirits which rises from the cauldron, and in passing this vapour through a series of vessels partly immersed in cold water, which make it deposit its aqueous particles, so that the spirit

of wine alone, very pure, is condensed in the last refrigerant.

In this way, instead of heating at first in order to obtain spirits of 19 degrees, from which we afterwards produced by successive heats spirits of different strengths, we have all at once any degree of strength we please. Besides, the old-fashioned alembic only received two charges per diem, whereas that of Adam receives eight, and it extracts a sixth part more spirit from the same quantity of wine: it saves two fifths of the combustibles and three-fourths of the manipulation, and finally the spirit which it furnishes has never an empyreumatic smell.

With such advantages, it is not to be wondered that the process in question has been so speedily and generally adopted. M. Duportal, a chemist of Montpelier, has presented to the Institute a very accurate description of it, which has been printed, and in which he also points out certain improvements which have been suggested by M. Isaac Berard.

It is essential to notice, that the primitive idea of heating by vapour belongs to Count Rumford, who published it in London in 1798. It is thus that a simple general proposition, which at first sight was regarded as an abstract truth and without any useful application, may enrich whole provinces.

Count Rumford, who has made so many useful discoveries, and who has made the œconomizing of heat so peculiarly his study, has this year presented to the Class several useful memoirs upon lights.

After describing various new forms of lamps, adapted for decorating apartments and serving for chambers, lanterns, &c. without any of the inconveniences generally complained of, he endeavours to resolve the great question which has divided natural philosophers for upwards of a century, viz. —Is light a substance which emanates from luminous bodies? or is it a movement impressed on these bodies by a fluid in other respects imperceptible, and diffused throughout space?

As a given quantity of a given species of combustible always gives out in burning one and the same quantity of heat, it ought also, according to Count Rumford, to give out one and the same quantity of light, if the light were contained in it in the same way as heat is: for those even who do not consider heat as a substance, admit that it is a force, a quantity of movement which may be concentred in a body, and which issues from it in the same quantity in which it was placed in it, as a spring unrolls itself. On the

On the contrary, if light is only a motion impressed on air, by the vibrations of the bodies which burn, its quantity will be in proportion not to the quantity of this body which shall have been burned, but to the vivacity with which the combustion shall have been effected, and particularly at the time that each of its particles shall remain heated at the degree proper for giving an impulse to those of air.

Having founded his experiments upon these ideas, with lamps as well as candles, he found that the heat emitted in a given time was always in proportion to the quantity of oil or wax burnt, while the quantity of light furnished at the same time varied in an astonishing degree, and depended particularly on the size of the flame, a size which retards its cooling: a small rush-light, for instance, gives sixteen times less light than a common taper, although burning as much wax and heating the same quantity of water to the same degree.

Thus every thing which supports the heat of the flame contributes to increase the light, and we may attain some most astonishing results.

Count Rumford, who had ascertained by previous experiments that every flame is transparent with respect to another flame, has combined his two discoveries; and having constructed lamps in which several flat wicks placed parallel to each other, mutually preserved each other from cooling, he made them produce a light equal to forty tapers; and he thinks the intensity which we may reach has no bounds: formerly it was considered impossible to carry the effect of light beyond a certain length, because, by enlarging too much the wicks with a double current of air, their light diminished in virtue of the causes to be accounted for by the following experiments:—

What we have said above of the cooling of bodies by evaporation, is a particular case of the law according to which every body which is dilated absorbs heat, whereas it is liberated by condensing. This law is nevertheless subject to some exceptions, and some of them have been long since known and explained: such as those of nitre, which preserves on many occasions, in condensing, a great proportion of heat, the effects of which are sufficiently perceptible at the moment of the combustion of gunpowder; but there are also some of these exceptions which depend upon more obscure causes, such as that which has been made known by M. Thillaye, professor in the Imperial School.

The mixture of alcohol with water is always accompanied by a rise in the temperature, and there is in general a

stronger condensation effected, than there would be according to the proportional densities. of the two fluids, a condensation according to which we can account for this heat.

But M. Thillaye has found, that when the alcohol is weak, so far from the mixture condensing, it is rarefied, and nevertheless the heat is manifested in the usual manner. He has constructed tables of his experiments, from which we see that alcohol at 0·9544 of density begins to exhibit rarefaction. The maximum of the effect is shown when the alcohol is at 0·9688, and when we mix it with one and a half its weight of water, and the elevation of temperature is still two degrees.

The contrary case, that of condensation of heat, produces detonating substances, the best known of which is gunpowder. One of the most dreadful is that kind of powder in which we substitute instead of the nitre the oxygenated muriate of potash; but it is also one of the most dangerous, for it detonates on simple percussion, and even by friction. It has nevertheless been considered as calculated for the priming of fire-arms, because, as it needs no spark of fire, it never fails.

Messrs. Bottée and Gengembre have contrived a powder, which preserves the property of detonating by a shock, without being liable to the danger of a spontaneous explosion. It is composed of 44 parts in the 100 of hyperoxygenated muriate, 21 of common nitre, or nitrat of potash, 18 of sulphur, and seven of powder of lycopodium. It requires the shock of the hardest bodies, and, what is more singular, the part only which sustains the blow detonates: the adjoining parts are only inflamed by communication, but they produce no explosion, so that this powder is absolutely harmless. It is important therefore, since it renders easy the use of a process which it has of itself.

The inquiries of chemists to discover substitutes for colonial produce continue to be carried on with great zeal.

Our associate M. Deyeux has published a string of instructions on the culture of beet-root, with a view to render it more productive of saccharine matter. M. Zanette has presented some experiments on the saccharine quality of the juice of maize. M. Deslongchamps, a physician of Paris, has made some experiments on the effects of the juice of garden poppy compared with the opium of the East. He has found them similar, so far as regards the juice obtained by the incision of the capsules, but twice as weak

weak with respect to the juice obtained by expression, and four times weaker in the extract from the leaves and stalks: the first only has the peculiar odour on which the bad effects of opium are thought to depend.

M. Chevreul, assistant at the Museum of Natural History, has made experiments upon pastil, with a view to illustrate its effects as a substitute for indigo; or rather, he has made this interesting plant the object of researches still more general, and better adapted for perfecting all the methods of vegetable analysis. He has shown that the feculum of the pastil is composed of wax, and of a combination of green resin, of a vegeto-animal matter, and of an indigo in the state of deoxidation, but which may easily again take back its oxygen. The filtered juice has also furnished him with substances the number and variety of which are astonishing, and from which we may conclude, that some of those which we have hitherto regarded as the immediate principles of vegetables, may be divided without decomposition into more simple principles.

The same chemist has presented a similar series of experiments on Campeachy wood. He has discovered in it fifteen different principles, the most remarkable of which is that which he has called *campechium*, and to which this wood owes its dyeing properties. This principle is a reddish brown, without taste or smell; it crystallizes; gives out upon being distilled the same elements with animal substances; is combined with all the acids and all the salifiable bases; and forms with the first of these substances red or yellow combinations, according to the quantity of acid employed, and with the others violet blue combinations; and that with the more facility, as we may employ it with more safety than the syrup of violets, in order to find out the alkalis; but the oxide of tin at the maximum forms an exception to this rule. It acts upon campechium like an acid, and reddens it; whereas the sulphuretted hydrogen, which under other circumstances acts like the acids, takes the colour from campechium.

Hitherto the theory of affinities had been applied only to the reciprocal decomposition of the soluble salts: it remained to be seen, if the insoluble salts were also susceptible of changing principles with certain soluble salts.

M. Dulong has examined this question in a general manner, in a memoir presented to the Class, and which is the first production of this young chemist. He first treats in particular of the action of the carbonates and of the subcarbonates of potash and soda on all the insoluble salts,

and he attains this remarkable result: viz. that all the insoluble salts are decomposed by the above two carbonates; but that the mutual change of their principles cannot be completely effected in any case; and reciprocally, that all the soluble salts from which the acid may form an insoluble salt with the base of the insoluble carbonates, are decomposed by the latter, until the decomposition has attained a certain limit, which cannot be exceeded; so that, in identical circumstances, combinations are produced absolutely opposite in their natures. M. Dulong observes, that there is perhaps no fact more evidently contradictory to Bergman's theory of affinities. He founds the explanation which he gives of these phænomena, in appearance contradictory, upon the changes which take place during decomposition; in the degree of saturation of the alkali, which is always in excess, and forms a new application of the principle so well established by M. Berthollet, upon the influence of the mass in chemical phænomena. Finally, he deduces from this theory a method of foreseeing what are the soluble salts susceptible of decomposing any given insoluble salt.

The celebrated Scheele discovered in 1780, that Prussian blue is only a combination of iron with a particular acid which the chemists have since called *Prussian acid.* It had not hitherto been obtained mixed with abundance of water. M. Gay Lussac in decomposing the prussiate of mercury with the muriatic acid by the aid of heat, by rectifying the product in flasks immersed in ice, and by rectifying it over carbonate and muriate of lime, succeeded in giving to the prussic acid the highest degree of concentration. In this state, this acid possesses remarkable properties. Its smell is almost insupportable; and, what is more singular, it boils at a heat of 26 degrees and freezes at 15; an interval so inconsiderable, that when we place a drop upon a sheet of paper, the evaporation of part produces a sufficiency of cold to freeze the remainder.

M. Boullay, a chemist residing at Paris, to whom we are indebted for the discovery of a phosphoric ether, has also formed one with alcohol and arsenic acid: but for this purpose abundance of these substances must be employed. The properties of this ether are similar to those of sulphuric or common ether, and the theory of its formation is the same.

M. Chretien, a physician of Montpelier, having discovered in certain preparations of gold, some very remarkable properties in the cure of syphilitic and lymphatic diseases, the attention of chemists has been directed to this metal,

metal, and Messrs. Vauquelin, Duportal, and Pelletier have again examined these solutions, in order to acquire a more precise knowledge of the state in which it exists in the pharmaceutical preparations: there nevertheless remained much uncertainty on this subject, because the chemical properties of several of the combinations of gold are very purgative.

M. Oberkampfs jun. has presented this year to the Class a maiden performance on a chemical subject, in which he has dispelled some errors. He has produced sulphurets and phosphurets of gold, and shows that the astonishing differences, observed in the action of the alkalis on the solutions of gold, depend on the proportion of the alkali: if there be enough of it, the precipitate is black, and it is a true oxide of gold: if there is not enough, the precipitate is yellow, and it is a muriate with excess of oxide: the difference of proportion of the acid does not produce less varied effects. Finally, in the precipitation by the oxide of tin, the results differ still more, according to the proportion of the oxide. M. Oberkampfs has determined the quantity of oxygen contained in the oxide of gold, and which is such, that in 100 parts there are 90·9 of gold and 9·1 of oxygen.

Our associates Messrs. Thenard and Gay Lussac have printed this year their Physico-chemical Researches, in which they have collected all the memoirs which they have read to the Class up to the present period, besides a great many others, all of them more or less important for the sciences which these young chemists cultivate with so much advantage.

Messrs. Bouillon Lagrange and Vogel have published a French translation of Klaproth's Dictionary of Chemistry, a work which in a small compass contains all the essential points in chemistry, detailed with as much clearness as solidity, and according to the newest discoveries.

Meteorology.—Since the fall of stones from the atmosphere has become the subject of investigation, they have been more frequently observed. General Count Dorsenne has sent us from Spain a meteorolite which fell in Catalonia. M. Pictet, a corresponding member, has furnished us with an account of two others, one of which fell on board of a ship; a novel circumstance.

M. Sage, taking occasion to describe some water-spouts which have been more frequent than ever this year, has collected in a detailed memoir a history of all the known phænomena of this description from the remotest ages.

XII. *Intelligence and Miscellaneous Articles.*

On Vaccination.

In our xxxixth volume, p. 152, we called the attention of our readers to a melancholy history of the ravages of the small-pox in Norwich, as recorded in an Address to the Corporation of Guardians of the Poor of that city, delivered Jan. 6, 1812, by Mr. Rigby; from which it appeared that at the close of the year 1807, a poor woman in the eruptive stage of the disease, having been brought to the city in the London waggon, and the judicious means proposed by him for preventing its spread being neglected by those who had authority to carry them into effect, a great number of persons, probably more than twelve hundred, caught the infection, of whom two hundred and three died. Notwithstanding the temporary failure of the endeavours of this benevolent and patriotic magistrate to persuade the corporation to adopt such necessary measures, we have great pleasure in finding from the following Narrative *, that they have been at length induced, on a re-appearance of the disease, to carry them into effect with complete success.

I HAVE much satisfaction in annexing the following account of the successful issue of my last application to the Court of Guardians, on the subject of small-pox †, and in recording the extensive benefit which has already resulted to the city from the adoption of the simple and obvious measures suggested by me; and which, whether considered with regard to the quantum of human life, in the first instance, unquestionably saved by it, or as having established a practical fact, of no small importance, as it bears relation both to the healing art and to the useful science of political œconomy; or, further, as it may excite others to have recourse to similar means of security against a loathsome and destructive disease, cannot be uninteresting to humanity.

Having learned in July last (1812), that the small-pox had, in the preceding Whitsun week, been introduced into Acle, a small town about eleven miles from Norwich, being brought thither by a young man from London, who had been incautiously discharged from the Small-pox Hospital,

* Appendix to "Further Facts relating to the Care of the Poor in the City of Norwich, by Edward Rigby, Esq. F.L.S. Senior Surgeon of the Norfolk and Norwich Hospital."

† This was the sixth time I had endeavoured to direct the attention of the court to this important subject; and the result should encourage every one who advocates the cause of humanity to persevere, even against the most discouraging opposition.

pital, whilst he carried about him, on his person and his clothes, the means of infection; and that it had found its way to the several villages of Blofield, Strumpshaw, Plumpstead, &c. more nearly in the vinicity of Norwich; I attended the monthly meeting of the Guardians on Tuesday, August 4, 1812, for the purpose of making this fact known to them, and of representing the danger of receiving the infection, to which the poorer inhabitants of the city wonld, probably, be exposed. The number of gentlemen present was not sufficient to constitute a court. The majority however concurred in the propriety of directing the attention of the public to the subject, and the following paper was ordered to be circulated:

"SMALL-POX.

"The corporation of Guardians of the Poor, in this city, having received information that the small-pox prevails much in the neighbourhood of Norwich, and that there is every reason to fear that it may soon find its way into the city, and great numbers of the children of the poorer inhabitants being liable to take the infection, the court earnestly recommends that all such children should be immediately vaccinated, and for this purpose the city surgeons have received directions to vaccinate all who may apply to them, without any expense. And to induce the parents of such children to comply with this recommendation, the court thinks it right to state, that when the small-pox last visited the city, about three years ago, more than two hundred individuals were sacrificed to it; which calamity might have been averted, had a similar measure to that now recommended taken place at that time.

"Norwich, August 4, 1812."

This made a considerable impression on the inhabitants, and the early efforts of two gentlemen, who merit, on this occasion, the most respectful notice, contributed much to forward its important object. Mr. Deacon, one of the city surgeons, on seeing the paper, thought it right immediately to go round his district, with the hope of inducing the poor families to consent to vaccination, and he had soon the satisfaction of reporting to me more than forty individuals who were ready to undergo it; and the Rev. Mr. Talbot, minister of St. Mary's, also visited the poor in his parish; in doing which he found a case of small-pox, which he reported to me; and being convinced of the fact, I requested the governor to call a special court of guardians, for further discussing the subject, and adopting such means of securing the city from the disease, as the urgency of the circumstances seemed to demand.

The

The court met on the 13th of August 1812, and was well attended. I stated the fact of the small-pox being in the city—that I was convinced there were more than a thousand poor children in it liable to take the infection—that vaccination was the most obvious, practicable, and efficacious means of securing them—that I had reason to believe the prejudices of the poor against it had much subsided *; which belief, derived, in some degree, from my own intercourse with them, had been much strengthened by Mr. Deacon's recent report; and wishing this favourable disposition of the poor to be taken advantage of, I suggested the policy of increasing the motive to their consent to vaccination, by a small pecuniary gratification; and as a further means of preventing the more immediate communication of the disease from those who might then labour under it, I recommended that the regulations suggested at Chester, many years ago, by Dr. Haygarth, might be adopted. After a candid discussion, the unanimous resolution of the court was made known by the immediate publication of the following paper:—

" *City of Norwich, and County of the same.*

" At a special court of the governor, deputy governor, assistants, and guardians of the poor, in the said city and county of Norwich, and liberties of the same, held at the new Hall, in the said city, the thirteenth day of August, in the year of our Lord one thousand eight hundred and twelve, to take into consideration the best means of preventing the spread of the small-pox, which has made its appearance in this city;

" *Resolved*—That the following Regulations to prevent the spread of the small-pox be printed and circulated, together with the last Report of the National Vaccine Establishment, printed by order of the House of Commons; and that a room in the workhouse should be set apart for the reception of any person who may be infected with the small-pox, and who may be consenting to be removed thither.

" *First.*—Suffer no person who has not had the small-pox or cow-pox to come into the infectious house. No visitor, who has any communication with persons liable to the distemper, should touch or sit down on any thing infectious.

* I have ever been convinced, when time and repeated experiment had unequivocally established the efficacy of vaccination, and the poorer classes had fairly witnessed the security it gives against the small-pox, that their prejudices respecting it would cease, and they would as readily avail themselves of this "kind gift of Providence" as other classes have done, and who have adopted it earlier only because they were sooner within the reach of that information, and those facts, which were equally necessary to their conviction.

" *Second.*

" *Second.*—No patient, after the pox have appeared, must be suffered to go into the street or other frequented place.

" *Third.*—The utmost attention to cleanliness is absolutely necessary. During and after the distemper, no person's clothes, food, furniture, dog, cat, money, medicines, or any other thing that is known or suspected to be daubed with matter, spittle, or other infectious discharges of the patient, should go out of the house till they be washed, and till they have been sufficiently exposed to the fresh air. No foul linen, or any thing else that can retain the poison, should be folded up and put into drawers, boxes, or be otherwise shut up from the air, but immediately thrown into water and kept there till washed. No attendants should touch what is going into another family till their hands are washed. When a patient dies of the small-pox, particular care should be taken that nothing infectious be taken out of the house so as to do mischief.

" *Fourth.*—The patient must not be allowed to approach any person liable to the distemper till every scab is dropt off; till all the clothes, furniture, food, and all other things touched by the patient during the distemper, till the floor of the sick chamber, and till his hair, face, and hands have been carefully washed. After every thing has been made perfectly clean, the doors, windows, drawers, boxes, and all other places that can retain infectious air, should be kept open till it be cleared out of the house.

" *Resolved*—That a reward of half-a-crown be given to every poor person resident within the city of Norwich, who shall be vaccinated by the city surgeons, at the Norwich dispensary, or in any other way, provided they produce to the committees a satisfactory proof of the fact.

" *Resolved*—That the thanks of this court be given to Edward Rigby, Esq. for his unremitting attention to the important subject of the small-pox—for the measures now proposed by him, and adopted by this court, in consequence of the disease being at this time in Norwich; and particularly for the able manner in which he has advocated the practice of vaccination, and so satisfactorily obviated the popular objections to it ——By the court, SIMPSON."

The report of the National Vaccine Establishment being in so many hands, and being moreover a parliamentary record, it has not been thought necessary to reprint it.

The vaccination began immediately, and the readiness with which the poor submitted to it is proved by the following returns, which appeared in the Norwich papers:

VACCI-

1812.	VACCINATED.		
Aug. 10 to 27.	By Mr. Keymer, city surgeon	17	
	Robinson, do.	69	
	Deacon, do.	116	
	Rigby,	77	—279
Aug. 27 to Sept. 3.	Keymer 5, Robinson 57, Deacon 74, Rigby 39, at the Dispensary by Mr. Powell 52,		227
Sept. 3---10.	Keymer 5, Robinson 58, Deacon 94, Rigby 38, Cooper* 38, Powell 15,		248
Sept. 10—17.	Keymer 2, Robinson 62, Deacon 69, Rigby 30, Cooper 15, Powell 8,		186
Sept. 17—24.	Keymer 6, Robinson 29, Deacon 75, Rigby 17, Cooper 16, Powell 13,		156
Sept. 24—Oct. 1.	Keymer 7, Robinson 17, Deacon 23, Rigby 31, Cooper 21, Powell 6,		105
Oct. 1—8.	Keymer 2, Robinson 11, Deacon 14, Rigby 9, Cooper 15, Powell 7,		58
Oct. 8—15.	Deacon 17, Rigby 5, Cooper 8, Powell 3,		33
Oct. 15—22.	Keymer 2, Deacon 6, Rigby 8, Cooper 2,		24
	Total number† vaccinated		1316

Of these, 944 have received the reward, the sum of 124*l.* 5*s.* having been paid by the court to this day, Oct. 26, 1812. 361 of these belonged to the country; for it was the liberal policy of the court to make no distinction between aliens and those belonging to Norwich: it was, indeed, obviously requisite to vaccinate all, for the security of all.

Means having been taken to prevent communication with the child who brought the disease from the country, no one caught it from this source; and if the same measures be persevered in, we may confidently calculate upon a permanent security from it. Compared with the population of the place, I believe in no instance, in this country at least, have so many individuals been vaccinated in so short a period; and the immediate consequent exclusion of small-pox when more than 1300 individuals were previously liable to it, is at once an irrefragable proof of the protecting power of vaccination, and of the magnitude of the blessing bestowed by Providence in its discovery.

P. S. Not a single case of small-pox has occurred to this time—Norwich, Dec. 27, 1812. E. R.

* The author of "Vaccination Vindicated," a well-written pamphlet of 64 pages, which has obtained much approbation from Dr. Jenner. He volunteered his services on this occasion, and vaccinated 119 children.

† The number vaccinated to December 27 is 1410.

CITY

CITY OF LONDON TRUSS SOCIETY.

The following statement of the situation and occurrence of hernia, at different periods of life, has been extracted from the register of the patients relieved by the City of London Truss Society* within the short period of five years.

In 4370 patients 3526 were males, and 844 were females.

Males.	Females.			
855	1	left inguinal	2323 inguinal	2694 single
1466	1	right inguinal		
15	177	left femoral	371 femoral	
13	166	right femoral		
1053		double inguinal	..	1138 double
13	72	double femoral		
14	201	umbilical	..	241
5	21	ventral		
15	24	have undergone operations, all of which have been completely successful		39
31	39	with umbilical hernia have been cured without trusses		70
46	7	with prolapsus ani		53
	135	with prolapsus uteri		135
3526	844	——4370		4370

283	patients were relieved with trusses under 10 years of age.					
215	ditto, between	10	and	20	ditto	
428	ditto	—— 20	and	30	ditto	
750	ditto	—— 30	and	40	ditto	
803	ditto	—— 40	and	50	ditto	
861	ditto	—— 50	and	60	ditto	
559	ditto	—— 60	and	70	ditto	
228	ditto	—— 70	and	80	ditto	
18	ditto	—— 80	and	90	ditto	
2	ditto	—— 90	and	100	ditto	
4147						

* *Plan of the Institution.*—The objects of this charity are to provide trusses for every kind of rupture—to furnish bandages and other necessary instruments for all cases of prolapsus—to perform every necessary operation—to administer surgical aid promptly—and to supply medicines and attendance during the cure of the patient.

Annual subscribers of one guinea, and life subscribers of ten guineas, shall be governors, and have the privilege of recommending three patients within the year.

The moneys arising from all life subscriptions are regularly invested in the public funds.

Patients relieved by this Society in	1808 - - -	227
	1809 - - -	570
	1810 - - -	813
	1811 - - -	1094
	1812 - - -	1666
		4370

Four females each had umbilical and double femoral hernia, five females each had umbilical and single femoral hernia.

Four males each had left femoral and right inguinal hernia, one male had left inguinal and left femoral hernia, one male had left inguinal and right femoral hernia, one male had right inguinal and left femoral hernia, one male had double inguinal and right femoral hernia, one male had double inguinal and double femoral hernia, two males had ventral and right inguinal hernia, one male had umbilical and left inguinal hernia, and four males each had umbilical and double inguinal hernia; 315 patients had congenital hernia.

From the most accurate estimation, it appears that this malady exists in one person in eight through the whole male population of this kingdom, and even in a much greater proportion among the labouring classes of the community, in manufacturing districts, particularly in those persons who are employed in weaving, or on the water as boatmen.

21, *Greville-Street, Hatton Garden,* 31*st Dec.* 1812.

JOHN TAUNTON,

Surgeon to the city of London Truss Society, the City and Finsbury Dispensaries, and Lecturer on Anatomy and Surgery.

METEOROLOGICAL TABLE,

Extracted from Lord Gray's Register kept at Kinfauns Castle, three miles almost due E. from Perth, N. Britain, about 90 feet above the level of the river Tay.—Lat. 56° 24′.

1812.	Morning, 8 o'clock. *Mean height of*		Evening, 10 o'clock. *Mean height of*		Depth of Rain.	N° of Days.	
	Barom.	Ther.	Barom.	Ther.	In. 100	Rain or Snow.	Fair.
January.	29·92	30·60	29·94	30·70	·72	7	24
February.	29·64	37 55	29·67	37·31	2·16	17	12
March.	29·96	33·46	29·99	33·30	86	17	14
April.	30·09	38·40	30·11	36·71	1·38	8	22
May.	30·02	48·20	30·02	45·45	1·41	17	14
June.	30·01	54·17	30·02	52·00	2·89	12	18
July.	30·04	55·22	30·05	52·97	2·56	15	16
August.	30·09	55·10	30·10	53·16	2·33	11	20
September.	30·03	52·00	30·03	49·00	1·06	6	24
October.	29·47	45·00	29·50	45·10	3·18	16	15
November	29·89	37·76	29·91	38·10	3·50	14	16
December.	30·09	34·00	30·14	35·00	·65	9	22
Average of the year.	29·937	43·43	29·953	42·40	22·75	149	217

Meteorological Observations made at Clapton, from January 1 to 17, 1813.

Jan. 1.—Cloudy, foggy, and dark days. Wind southerly and calm. Thermometer highest in day 45; lowest in night 40.

Jan. 2.—Cloudy and misty, some large indistinct *cirro-cumulus:* stars shone by nine at night.

Jan. 3.—Thick fog in the morning.

Jan. 4.—Cloudy and damp all day.

Jan. 5.—Clouds and misty; wind rising towards night. Maximum of thermometer happened at night, during which the warmth increased.

Jan. 6.—Gale from S.W. and warmer, with clouded sky and misty air. Though the sky overhead was overcast, yet I could discern loose portions of cloud floating along in the wind, which increased, and by night became high. Night very dark.

Jan. 7.—Foggy and calm in the morning: the barometer fell during the day, and was followed by wind and rain in the evening. Very dark night. S.

Jan. 8.—Foggy early; day became fair with low fleecy *cumuli* flying along in the wind, afterwards more elevated, and flat masses took on in part the form of *cirrocumulus*—at a later period of the day, rainy features of *cirrus*, *cirro-stratus* with *cumulostratus:* haze and a gentle gale from S.W. Frosty night.

Jan. 9.—White frost and fog followed by rain, afterwards clear with rainy features of *cirrus* and *scud.* Frosty night again, a *halo* round the moon *.

Jan. 10.—Clear frosty day.

Jan. 11.—Frosty morning, afterwards some little clouds put on the cirrocumulative form, and *cirrocumulostratus* followed. About half past two P.M. a small balloon launched from Clapton went in a direction to the northward; wind therefore southward.

Jan. 12.—Cloudy and raw, though with a southern wind.

Jan. 13.—Cold and raw, with S.E. wind; some sleet fell in the day. Fair night.

Jan. 14.—Cloudy and damp; some slight showers of snow fell at night. S.E—E—N.E.

Jan. 15.—Cloudy and damp. S.E.

Jan. 16.—Frosty and foggy in the morning; clear fine day, with features of *cirrus* and *cumulus.*

Jan. 17.—Cloudy and cold, with S.E. wind.

Clapton, Jan. 18, 1813. THOMAS FORSTER.

* By a *halo* I do not mean a mere *corona* or disk, but a ring. See Phil. Mag. for 1812, wherein I have classified these phænomena.

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For January 1813.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Dec. 26	30	34	33	30·40	4	Cloudy
27	32	34	31	·47	0	Cloudy
28	30	36	36	·45	6	Cloudy
29	39	46	43	·15	10	Cloudy
30	43	47	42	29·90	14	Fair
31	42	45	42	·81	0	Small Rain
Jan. 1	40	46	40	·83	0	Cloudy
2	42	47	42	·99	0	Cloudy
3	39	42	39	30·26	10	Fair
4	40	43	38	·12	0	Cloudy
5	38	42	40	·01	0	Cloudy
6	40	48	40	29·70	0	Wind and Rain
7	41	46	42	·60	0	Small Rain
8	47	50	36	·55	16	Fair
9	34	41	35	·70	10	Fair
10	33	38	32	·90	14	Fair
11	33	36	33	·86	0	Sleet and Rain
12	34	37	34	·60	0	Foggy
13	33	38	34	·50	0	Small Rain
14	34	37	33	·62	0	Cloudy
15	34	38	30	·79	0	Cloudy
16	30	43	34	·90	11	Fair
17	33	37	32	30·15	17	Cloudy
18	29	34	32	·04	7	Cloudy
19	32	33	32	·24	6	Cloudy
20	31	34	30	·17	7	Cloudy
21	32	35	33	·24	8	Cloudy
22	31	33	32	·39	8	Cloudy
23	26	33	33	·19	5	Do. with Snow
24	30	34	29	·30	6	Cloudy
25	28	35	33	·40	0	Foggy
26	34	38	34	·35	0	Cloudy

N. B. The Barometer's height is taken at one o'clock.

XIII. *An Attempt to determine the definite and simple Proportions, in which the constituent Parts of unorganic Substances are united with each other.* *By* JACOB BERZELIUS, *Professor of Medicine and Pharmacy, and M.R.A. Stockholm.*

[Continued from page 8.]

II. LEAD AND SULPHUR.

1.) TEN grammes of very pure lead were melted, in a small glass retort, with ten grammes of pure lemon-coloured sulphur, which had been carefully sublimed and melted in a strong heat, in order to expel the moisture. The opening of the retort was connected with a small pneumatic apparatus, but no perceptible quantity of gas was emitted, except a little sulphurous acid gas, which had occupied the place of the oxygen that had disappeared. The mass was ignited until the yellow colour, produced in the retort by the sulphurous acid fumes, was no longer visible: and while the apparatus cooled, some water was absorbed in the place of the air that had been forced out. I cut off the body of the retort, and found the weight of the sulphuret 11·55 gr.

2.) A repetition of the experiment gave 11·555 grammes of the sulphuret.

3.) The experiment was repeated with the additional precaution of burning a little sulphur in the receiver cemented to the retort, and strongly heating it, before the retort itself was warmed, so as to deprive the air in the receiver of its oxygen. The retort was heated till it began to bend. The sulphuret weighed 11·56 grammes. Consequently 100 parts of pure lead take up 15·6 of sulphur, or exactly twice as much as of oxygen. I have not been able to discover any other sulphuret of lead than this. We have therefore for the sulphuret of lead,

Lead	86·51	100·0
	13·49	15·6

Wenzel, on Affinities, gives 86·8 and 13·2 for the proportion of the lead to the sulphur.

III. SULPHUR AND OXYGEN.

Klaproth, Bucholz, and Richter have very accurately examined the proportion of oxygen in the sulphuric acid, and their experiments agree extremely well with each other. But it became so much the more necessary to repeat these

 experi-

experiments, as Davy had conjectured that sulphur and phosphorus contained unknown metallic bodies, united with small portions of hydrogen and oxygen, and bore nearly the same relations to these bases as the resins do to carbon. He has indeed advanced so many ingenious arguments in favour of this hypothesis, that it can scarcely be called altogether void of probability: at the same time I cannot consider it as perfectly consistent with my experiments: for, when I have used sulphur that was perfectly dry, and had been enough melted, I have never been able to discover any trace of sulphureted hydrogen, or of aqueous vapour, emitted during the combination of metals with sulphur. But I have often observed, that when I have employed washed and apparently well dried or rapidly melted flowers of sulphur, no moisture made its appearance while the sulphur remained melted over the metal; but when, at the moment of combination, the heat became more intense, a small quantity of sulphureted hydrogen has been evolved, and aqueous vapour has been deposited on the glass, a little sooner than the sulphur which was sublimed at the same time. Consequently, either the oxygen and hydrogen found by Davy depended only on the accidental presence of moisture, or they enter, together with the unknown basis, into combination with the metals; which is much more contrary to analogy than the parallel between sulphur and the resins is supported by analogy. We shall also see that these substances, if they are present in sulphur, must be retained when it combines with oxygen to form an acid. Sulphur and phosphorus, and, as I suspect, boracium, fluorium, and carbon, give, with the metals, combinations of a very different character from those of the metals with each other. They cannot be alloyed in all possible proportions with metals, but are limited either to a single proportion, or to a few definite degrees, between which there are no intermediate steps.

A. *Sulphuric Acid.*

In order to avoid all mechanical adherence of moisture, I employed in this investigation the sulphuret of lead.

1.) Ten grammes of finely powdered sulphuret of lead were digested with aqua regia in a weighed glass flask, as long as any oxygenization was perceptible; and the mass was then dried and ignited: it weighed 12·65 gr. It was afterwards digested with water, to which a little concentrated vinegar had been added; but the liquid acquired no sweet taste, and contained no lead. Consequently the sul-

phur

phur of the sulphuret had been sufficient to form so much sulphuric acid, as was necessary for neutralising the oxide of lead.

2.) In a second experiment the residuum weighed 12·64 gr.

3.) The experiment was again repeated in a glass retort with a receiver, and the acid which passed over was poured back and distilled once more from the mass in the retort. The last part of the product was kept separate, but it afforded no perceptible trace of sulphuric acid; consequently the oxide of lead was sufficient to saturate all the acid which was formed from the sulphur of the sulphuret.

Hence I conclude, that since the sulphuret of lead contains its constituent parts precisely in the proportion, which is necessary for the formation of the sulphate, the oxide of lead must contain exactly half as much oxygen as the sulphuric acid contains sulphur. Probably the same rule holds good for the combination of sulphur with other combustible bodies: hence it will follow of necessity, that the quantity of any oxide required for saturating a given portion of sulphuric acid must contain half as much oxygen as there is sulphur in the acid; that is, if my experiments on the oxide and the sulphuret of lead have not been completely erroneous.

The quantity of sulphur in the sulphuric acid may be easily determined from these experiments. Ten grammes of the sulphuret of lead having taken up 2·65 of oxygen, ·675 of this belongs to the 8·651 of lead, the remaining 1·975 formed, together with 1·349 of sulphur, 3·324 of sulphuric acid. Consequently 100 parts of sulphuric acid consist of 40·58 sulphur and 59·42 oxygen. The second experiment gives 40·7 and 59·3: and 100 parts of the acid would require for their saturation, according to the first experiment, 280·5, and according to the second, 281 of the oxide of lead; a degree of coincidence which can scarcely be exceeded.

In order to ascertain what dependence can be placed on these results, it was necessary to examine the composition of the sulphate of lead.

a.) The 10·77 grammes of oxide of lead, obtained in the first of my experiments on the yellow oxide, were dissolved, in the same dish, in nitric acid, and then mixed with sulphuric acid as long as any precipitate could be observed; and afterwards dried and ignited. They afforded 14·62 gr. of sulphate of lead; consequently 100 parts of sulphuric acid had combined with 280 of the oxide.

 b.) Ten

b.) Ten grammes of lead were dissolved in nitric acid mixed with sulphuric, dried by evaporation, and ignited in the flask. The sulphate weighed 14·635, and 100 parts of the acid had combined with 280 of the oxide.

c.) Ten grammes of oxide of lead were dissolved in nitric acid, in a dish of platina, sulphuric acid was added, the mixture was then dried and ignited. I obtained 13·575 gr. of sulphate of lead, which gives the same proportion as before.

These three experiments, agreeing so perfectly with each other, indicate a small inaccuracy in the analysis of the sulphuric acid. This depends on the sublimation of a little of the sulphur of the sulphuret of lead employed, leaving the base of the salt somewhat redundant; the muriatic acid, which combines with this redundant portion, being expelled by ignition. Hence it is probable that the quantity of sulphur ought not to be made greater than 40·52 for 100 of sulphuric acid: but in order to avoid any hypothetical corrections, we may safely employ the first experiment as a basis for calculation, and consider sulphuric acid as consisting of

Sulphur	40·58	100·000
Oxygen	59·42	146·427

I must here remark, that Bucholz and Klaproth have founded their determinations on the quantity of sulphate of baryta afforded by a given quantity of sulphur: and as there is some difference in the results of their experiments, I have thought it right to repeat them.

In the carbonate of baryta, Klaproth and Rose found 22 parts of acid and 78 of earth; Bucholz, on the contrary, never obtained more than 21 per cent. of carbonic acid. The greatest difficulty is to obtain the carbonate quite pure, since it is so often contaminated with iron, alkali, or sulphuric acid. I have only been able to procure it by precipitation from the pure uncrystallized nitrate of baryta with carbonate of ammonia. I washed the precipitate with boiling water until it no longer indicated the presence of baryta upon the addition of sulphuric acid; for the carbonate of ammonia does not completely precipitate the whole of the baryta, even when it is employed in excess. The washed powder was strongly ignited in a dish of platina.

a.) Ten grammes of this carbonate were dissolved in diluted sulphuric acid, the apparatus being weighed, and the gas caused to pass through a tube filled with muriate of lime, which was also weighed. The solution was promoted

moted by the heat of a small lamp: but the fluid was not sufficient for the solution of the salt that was formed. After twelve hours, when no more bubbles were extricated, 2·11 grammes had been lost. I took the solution with the salt out of the flask, and mixed it, in a dish of platina, with sulphuric acid, when a slight effervescence again took place. The mass was then dried in a gentle heat, and afterwards ignited. It afforded 11·866 gr. of the sulphate.

b.) Five grammes, heated in a manner precisely similar, lost 1·08, and afforded 5·92 of sulphate of baryta. Hence, for 100 parts of the carbonate, we have 21·6 of carbonic acid, and 118·4 of the sulphate.

c.) Ten grammes of carbonate of baryta were dissolved, in the same apparatus, in diluted muriatic acid, and the solution assisted by such a degree of heat, as the hand could not bear, although it was not made to boil. They afforded 2·165 gr. of carbonic acid, and 11·82 of sulphate of baryta.

d.) Ten grammes of carbonate of baryta, dried in a press, and then ignited, so as to form hard lumps, which were more slowly dissolved, were heated in the same manner with muriatic acid. They lost 2·165 gr. and gave 11·86 of sulphate of baryta.

e.) The same quantity was dissolved in sulphuric acid, mixed with a little muriatic acid, in a glass flask, then dried and ignited in the same vessel. It afforded 11·89 gr. of sulphate of baryta.

f.) Ten grammes of carbonate of baryta were dissolved in muriatic acid in a glass flask, precipitated with sulphuric acid, evaporated, and ignited in the flask. They gave 11·9 gr. of sulphate of baryta. A stronger heat expelled nothing more from this compound. The acids which I have employed were always so pure as to leave no spot on a watch glass from which they were made to evaporate.

In these experiments, therefore, 100 parts of carbonate of baryta had afforded at least 21·6 of carbonic acid; ten thousandths can never be appreciated in experiments of this kind. We may therefore assume, for the carbonate,

Carbonic acid	21·6	100
Baryta......	78·4	363

Since 100 parts of the carbonate of baryta, which contain nearly 78·4 of the base, give from 118·6 to 119 of sulphate of baryta, this compound must consist of 33·96 to 34·1 of acid, with 66·04 to 65·9 of base, and 100 parts of the acid must require 193·0 to 194·5 of baryta. Since in the present state of these experiments an error of ·0005

 is

is of little consequence, I have assumed throughout this Essay, for the component parts of the *sulphate of baryta,*

Sulphuric acid	34	100
Baryta.......	66	194

If we took a mean of the six experiments which have been related, giving 118·627 of sulphate of baryta for 100 of carbonate, the proportion would become

Sulphuric acid	33·9	100
Baryta	66·1	195

Klaproth obtained, from 100 grains of carbonate of baryta, 120 of dry sulphate, and Bucholz $119\frac{1}{2}$, which were reduced by ignition to 117. Hence Klaproth calculated that the salt consisted of 33 acid and 67 base, Bucholz 32·48 and 67·52. They both employed precipitation and filtration. In the process of filtration it is scarcely possible to avoid loss, and the different degrees of moisture in the filter cause great uncertainties in its weight, since it cannot be weighed hot in a good balance, without giving a result considerably too small. I have therefore avoided this process as much as possible; but where it was indispensable, I have employed English copying paper, made for Watt's patent machines, which I have previously washed, and dried in as strong a heat as it could support without burning. The largest filters that I have used weighed less than ·75 gramme, and their weight has never varied more than ·006 gr., nor even so much, unless they were left very long in the scale. The smaller ones, which weighed from ·1 to ·25 gr. have never varied perceptibly. I have removed the mass lying on the filter, without scraping off the small quantity which was firmly attached to it; and having weighed and ignited that which I had removed, I have computed the diminution of the whole quantity by the operation.

Bucholz (Scher. X. 385,) boiled 100 grains of sulphur with aqua regia, until it was converted into sulphuric acid, from which he obtained 724 gr. of sulphate of baryta; and hence, according to his determination of the composition of this salt, it follows that 100 parts of sulphuric acid contain 42·5 of sulphur. According to my analysis, these 724 grains contain 246·16 of the acid; giving 146·16 of oxygen to 100 of sulphur; and, for 100 of the acid, 40·624 of sulphur and 59·376 of oxygen. Bucholz's experiment therefore agrees with mine, within ·00044. Bucholz employed sulphur which had been kept long melted in a strong heat; mine was ignited in combination with

with a metal; whence it follows that sulphur is easily freed by melting from the moisture which adheres to it.

With respect to Klaproth's analysis, it is not so accurate as that of Bucholz. He treated 200 grains of pure sulphur with nitric acid; $48\frac{1}{2}$ grains remained unaltered, the $151\frac{1}{2}$, which were oxygenized, afforded 1082 of sulphate of baryta, so that 100 parts of sulphur gave 15 less of the sulphate than in Bucholz's experiment: and yet the experiment was not repeated. At the same time it happens, from the different estimates which these chemists have formed of the composition of the sulphate of baryta, that they agree in their determinations of the quantity of sulphur in the sulphuric acid.

Richter's experiment (Richter v. 125) was performed in a different manner. He converted 222 grains of flowers of sulphur, by means of the smoking nitric acid, into sulphuric acid. The acid liquor was saturated with carbonate of lime, then dried and washed with alcohol and a little nitric acid, to separate from it the nitrate and carbonate of lime. The gypsum, when ignited, weighed 947 grains. Now, if 100 parts of this salt contain 58 of the acid, 947 must contain $549\frac{1}{4}$; so that 222 grains of sulphur must have taken up $327\frac{1}{4}$ of oxygen, and $100,147\frac{1}{3}$: hence 100 parts of the acid must contain 40·44 of sulphur, which agrees again very nearly with my experiments already related. But if the proportion of acid in gypsum has been taken a little too great, Richter's experiment approaches still nearer to mine. Bucholz obtained, in an analysis of 300 grains of gypsum, 63 grains of water of crystallization, 99 grains of lime, and 402 of ignited sulphate of baryta, from which the presence of 136·7 gr. of sulphuric acid may be inferred. These quantities, added together, make 298·7 gr. and the loss is only 1·3, while, if we reckon according to Bucholz's proportions, it becomes a little more than 6 gr. Bucholz, finding this loss pretty constant in several experiments, inferred from it that a part of the water adhered to the gypsum, notwithstanding the ignition. The component parts of gypsum, according to the proportions here assigned, are 58 of acid, and 42 of lime; but it is probable, that in the analysis performed by Bucholz, a loss of lime also took place, by which the proportion of the acid to the base becomes too great. Klaproth found in ignited gypsum 57·63 of acid, and 42·37 of base.

B. *Sulphurous Acid.*

To determine the composition of the sulphurous acid by

 direct

direct experiments with burning sulphur, is a task of insuperable difficulty. I therefore chose rather to convert a sulphite into a sulphate by means of the nitric acid.

Neutral muriate of baryta was mixed with a solution of crystallized sulphite of ammonia, the precipitate was placed on a filter, and washed with boiling water till the water had no effect on a solution of silver; the mass was then dried by pressure between the folds of some thick blotting paper, quickly spread in a saucer, and dried in a warm stove. When I dissolved a little of this salt in muriatic acid, the fluid was not perceptibly turbid, so that scarcely any sulphate of baryta was contained in it.

1.) Three grammes of this sulphite of baryta were put into a flask; nitric acid was poured on them, and they were digested with it as long as any nitrous gas was evolved, and then dried and ignited in the flask. The mass weighed only 3·17 gr., it showed not the slightest trace of an excess of baryta. But 3·17 gr. of the sulphate of baryta contain $·66 \times 3·17 = 2·0922$ of the earth.

2.) Three grammes of the same salt were mixed with 30 of ignited yellow oxide of lead, and the whole was heated in a small glass retort, furnished with a long and well corked neck. The neck, in which the water of crystallization of the salt was collected, was cut off and weighed. By the evaporation of the water it lost ·0425 in weight. The water was entirely without taste.

The sulphite of baryta consisted therefore of

Baryta	209·22	69·74
Sulphurous acid	86·53	28·84
Water	4·25	1·42

3.) I again dissolved three grammes of the same salt in nitric acid, and when the effervescence was ended, added some nitrate of baryta, as a test of sulphuric acid, to the filtered solution; it did not become turbid, any more than another portion into which sulphuric acid was dropped; consequently the baryta is united with the same quantity of sulphur in the sulphite as in the sulphate, that is, with 20·9 for every 100 parts of the earth; and we shall see hereafter, that if there actually exists a combination between the base of baryta and sulphur, the proportion of this base, and of sulphur in it, must be the same as in the sulphate and the sulphite; and also, at least as I presume, in the sulphuret, and in the hydrosulphuret, or hydrothionate of baryta, although the experiments which I have made with these last combinations have not afforded me very satisfactory results.

Now,

Now, if three grammes of sulphite of baryta contain ·8653 of sulphurous acid, and 3·17 gr. of the sulphate, into which they are changed by oxygenization, imply the presence of ·4374 gr. of sulphur, the remaining .4279 gr. of the acid must be oxygen. Consequently 100 parts of sulphur take up 97·83 of oxygen, in order to form sulphurous acid; and this acid consists of

Sulphur	50·55	100·00
Oxygen	49·45	97·83

If we took for the sulphate 33·9 of acid and 66·1 of earth, the 3 grammes of sulphite would appear to contain ·8621 of acid, and ·4361 of sulphur; whence the proportions would become

Sulphur	50·59	100·00
Oxygen	49·41	97·69

[The author seems to have omitted to notice some other determinations, as having been deduced from inaccurate suppositions respecting the component parts of the sulphate of baryta. Mr. Chenevix converted 14·4 parts of sulphur, by means of nitric acid, into 100 parts of sulphate of baryta, to which he attributed only 23·55 of sulphuric acid, and hence concluded that the acid consisted of 61·5 of sulphur, and 38·5 of oxygen. But, if we employ Mr. Berzelius's determination of the proportions of the sulphate of baryta, it will appear that 14 4 sulphur must have afforded at least 34 parts of sulphuric acid; whence the sulphuric acid should consist of 42·4 of sulphur, and 57·6 of oxygen. But these experiments by no means agree with each other so well as those of Mr. Berzelius. Mr. Berthollet's analysis, in the Memoirs of the Institute for 1806, which gives ·5385 of sulphur, and ·4615 of oxygen, led Mr. Gay-Lussac, in his interesting investigations respecting the decomposition of the sulphuric compounds by heat, to the erroneous conclusion, that the sulphurous acid contains only 50·61 of oxygen to 100 of sulphur, instead of at least 95·86, which is the proportion determined by Berzelius. The same chemist, in his remarkable essay on the combinations of gases, has assigned to the sulphuric acid 42·016 of sulphur, and 57·984 of oxygen, and to the sulphurous, 52·083 of sulphur, and 47·912 of oxygen; but his data require some very considerable corrections.—GILBERT.]

Since 100 parts of sulphur are combined in the sulphurous acid with 97·83 of oxygen, and in the sulphuric with 146·427, the latter number being very nearly half as much more as the former, since $97·83+48·96=146·74$, it follows that the same quantity of sulphur takes up in the sulphuric

sulphuric acid half as much more oxygen as in the sulphurous. If we compare with this result the proportions of the combinations of lead, it may be proposed as a question for future examination, whether sulphur may not be capable of a lower degree of oxygenization than it exhibits in the sulphurous acid, or of a higher than in the sulphuric. [To be continued.]

XIV. *Observations on the Measurement of three Degrees of the Meridian conducted in England by Lieut.-Colonel* WILLIAM MUDGE. *By Don* JOSEPH RODRIGUEZ. *Communicated by* JOSEPH DE MENDOZA RIOS, *Esq. F.R.S.*

[Concluded from p. 30.]

FROM what has been above stated, it seems almost beyond a doubt that it is to errors in the observations of latitude, that the appearance of progressive augmentation of degrees towards the equator, as represented by Lieut. Col. Mudge in his paper, are to be ascribed, and that it is especially at the intermediate station at Arbury Hill, that the observations of the stars are erroneous nearly five seconds, notwithstanding the goodness of the instruments, and the skill and care of the observer. But, before I insist further on this head, I will answer one objection that may be made to the principles of the method that I have pursued in this memoir.

Those astronomers, who have hitherto undertaken the measurement of degrees of the meridian, have deduced their measures by simply dividing the linear extent by the number of degrees and minutes found by observation of the fixed stars taken at the two extremities of the arc. This is indeed the most simple that can be adopted; and it has the advantage of being independent of the elliptic figure of the earth, especially in arcs of small extent. The elements dependent on this figure, are too uncertain to be employed in calculating the angular intervals in the short distances between successive stations, even as a means of verification, without risk of committing greater errors than those to which astronomical observations can be liable. Accordingly one cannot safely make any use of it in cases where great accuracy is required.

I must admit the justness of this objection, and must therefore show the extent to which it really applies to the present subject.

In the first place, I may suppose, that in consequence of some

some fault in the instrument, with respect to vertical position, construction, or some accidental derangement, there is an error of some seconds in the observations of the fixed stars. How is this to be discovered? This is not to be done by comparing the value of a degree on the meridian, as deduced from these observations, with the results of other measurements in distant parts of the globe. For if we find that these degrees so taken do not agree in giving the same ellipsoid, we are not to attribute all the differences to irregularities of the earth, without supposing any error on the part of the observer, of his instrument, or of other means employed in his survey.

But this, in fact, is what has generally been done. It must, however, be acknowledged, that the majority of observers have not been in fault, as they could do nothing better; but too much reliance has been placed on the goodness of their instruments, their means, and other circumstances. It is true that irregularities of the earth and local attractions may occasion considerable discrepancies which are even inevitable; but before we decide that these are the real source of disagreement, we ought carefully to ascertain that there are no others.

But to return to our subject, of the English measurement. If the uncertainty which yet subsists, with respect to the exact figure of the earth and its dimensions, occasions some small errors in the calculation of the series of triangles, the sum of these errors will be found in the estimate of the entire arc, and will increase in proportion to the extent of the arc measured. Now, in the English measurement, we find exactly the reverse of this. For the difference between the results of calculation and observation is only 1″,38 on the whole arc; but is even as high as 4″,77 on one of the smaller arcs. So that, whatever error we may suppose to have been introduced into the calculation by assuming a false estimate of the spheroidity of the earth, or of other elements employed in the calculation, it is very evident that the zenith distances of stars taken at Arbury Hill are affected by some considerable error, wholly independent of these elements.

It was not till the date of the measurement of the meridian in France, that M. Delambre published and explained, with admirable perspicuity and elegance, all the formulæ and methods relative to the calculation of spheroids, and put it in the power of astronomers in general to make use of the elliptic elements in verifying the results of their observations. In the present state of science these

elements

elements are well known, and the errors that can arise from any uncertainty in them, are not so considerable as is generally supposed. The oblateness and the diameter at the equator are the only elements wanting in the calculation: for the purpose of seeing what effect our present uncertainty respecting them can have on the subject in question, I have employed three different estimates of the oblateness $\frac{1}{330}$, $\frac{1}{320}$, and $\frac{1}{310}$. With respect to the radius of the equator, that is ascertained with sufficient precision by the mean of the arc extending from Greenwich to Formentera, corresponding to latitude 45° 4′ 18″. The value of the degree in toises is 57010,5, and it is highly probable that in this estimate the error does not amount to so much as half a toise, as it is deduced from an entire arc of 12° 48′ between the two extremities, the latitudes of which have been determined with extreme care, and by a great number of observations.

The following are the logarithms of radius at the equator, which I have employed as adapted to each degree of oblateness, and opposite to them are placed the corresponding computed estimates of the entire arc between Clifton and Dunnose.

$\frac{1}{330}$ .... 6,5147,400 ..... 2° 50′ 21,972
$\frac{1}{320}$ .... 6,5147,485 ..... 2° 50′ 21,974
$\frac{1}{310}$ .... 6,5147,570 ..... 2° 50′ 21,976

so that the greatest difference is but 0″,38. Let us suppose it 0″,4, or even 0″,5, for the second calculation was made only by means of the western series of triangles, and the third only with the eastern; but even then the error arising from uncertainty in the elements is not half the difference we find between the results of computation and of observations of the fixed stars. It appears therefore, that these elements are by no means to be neglected as a method of verification; and in fact the quantity of 1″,38 is so small, that it is extremely difficult to ascertain this quantity with the very best instruments. Of this we shall find further proof hereafter; but as this discussion is not without its use, I shall enter into some details on this subject.

The measurement in Lapland was performed by means of a double metre, and with a repeating circle of Borda, sent by the National Institute of France. In order to see to what degree of accuracy the arc computed would agree with that obtained by observations of the pole star above and below the pole, I assumed an oblateness of $\frac{1}{320}$, and as logarithm of radius I had 6,5147500 expressed in toises and

and in round numbers. With these elements, and with the data to be found in the work of M. Svanberg, we have by the western series of triangles 5840″,196 and 5840″,138 by the eastern. So that the mean calculated arc is 1° 37′ 20″,167, while the arc observed was 1° 37′ 19″,566. The difference then is 0″,6 for the total arc, and 0″,37 for the mean degree, or 5,86 toises excess in the linear extent. One can never depend upon quantities so small as this; so that the agreement between the results of computation and actual observation, proves not only the skill of the observers and the accuracy of which their instruments admit, but also that the elliptic elements employed in the calculation are a sufficiently near approximation to the truth to be deserving of confidence.

In the viiith volume of the Asiatic Researches, published by the Society at Calcutta, are contained the details of another measurement performed in 1802, by Major William Lambton in Bengal, on the Coromandel coast. In this undertaking, which was executed with great skill and attention, Major Lambton employed Bengal lights as signals, chains for the linear measures, and a theodolite, and a zenith-sector made by Ramsden. The base measured was 6667,740 fathoms reduced to the level of the sea, and to the temperature of 62° Fahrenheit; and the stations were so chosen, that four of the sides of the triangles were almost in the same line, and nearly parallel to the meridian at the southern extremity of the arc, so that their sum but little exceeds its whole extent. The lengths of these arcs in fathoms reduced to the meridian are thus given in the memoir of Major Lambton.

AB 20758,13 north latitude of A 11° 44′ 52″,59
BC 17481,245
CD 22237,04 north latitude of E 13° 19′ 49″,018
DE 35246,43

From these data Major Lambton deduces the degree of the meridian to be 60435 fathoms, or 56762,3 toises. By applying to this the same elements as we did to the measurement by Svanberg, we have the entire arc measured equal to 1° 34′ 55″,896; so that the difference between the results of calculation and of the observations, is only 0″,532 for the whole arc, or 0″,337 for the mean degree. The elliptic hypothesis and observation agree more correctly in this instance, for the difference is rather less than in that of Lapland, although the two arcs are very nearly of the same extent. Thus the degree on the meridian measured in Bengal, in the latitude of 12° 32′ 21″ north, cannot be

supposed

supposed to exceed Major Lambton's estimate by more than 5,22 toises; and it is extremely difficult to speak with certainty to quantities so small as this.

The same observer also measured one degree perpendicular to the meridian, by means of a large side of one of his triangles cutting the meridian nearly at right angles, and of which he observed the azimuth at the two extremities. The data from which his results may be verified are these:

Length of the chord of the long side in English feet AB=291 197,20.

Azimuth of the eastern extremity A equal to 87° 0′ 7″,54 NW.

Azimuth of the western extremity B equal to 267° 10′ 44″,07 NW.

North latitude of A 12° 32′ 12″,27
North latitude of B 12° 34′ 38″,86.

With these data in the triangle formed by the long side, the meridian at B, and the perpendicular from B on the meridian at A, we have the chord of this last arc equal to 290845,8 feet, and the arc itself 290848,03 feet. By applying the method of M. Delambre, we find the azimuth of the extremity B less by 2″ than it was observed to be; so that we have no reason to suppose a greater error than one second in the observation of each azimuth, and it seems next to impossible to arrive at greater exactness.

The difference of longitude between the points A and B is 48′ 57″,36. With this angle and the co-latitude at A, we have in the spherical triangle right angled at the point A, the extent of the normal arc equal to 2867,330 seconds, and dividing its length in feet by this number, we have for the degree perpendicular to the meridian, at the extremity A, 60861,20 fathoms, or 57106,5 toises. Now these values are precisely what we find on the elliptic hypothesis, with an oblateness of $\frac{1}{320}$ or $\frac{1}{310}$; and in short, the correspondence between the hypothesis and the measures of Major Lambton is as complete as can be wished. Major Lambton, indeed, finds the degree on the perpendicular too great by 200 fathoms, but this arises from a mistake in his calculation.

Lastly, I shall apply the same method, and see how nearly the elliptic hypothesis agrees with the last measures taken in France, which merit the highest degree of confidence both with respect to the observers who have executed it, and the means which they had it in their power to employ. I have taken only the arc between Dunkirk and

and the Pantheon at Paris, from the data published by the Chevalier Delambre in the third volume of the Measurement of the Merdian. I employed the same elements and similar calculations to those made on the English arc. The oblateness of $\frac{1}{330}$ gives the difference between the parallels equal to 7883,615 seconds by the eastern series of triangles, and 7883,617 seconds by the western series. The mean of these 7883,616 may be taken as the true extent of the total arc.

The two other elements give for this quantity 7883″,621 and 7883″,493, or 2° 11′ 23″,6 and 23″,49, as the calculated extent of the arc. But the arc observed was 2° 11′ 19″,83, according to M. Delambre, and 2° 11′ 20″,85 according to M. Mechain; so that the least difference between the calculation and the observations will be 2″,64. M. Delambre is of opinion, that the latitude of Dunkirk, which is supposed to be 51° 2′ 9″,20, should be diminished; and in fact the distance between the parallels of Dunkirk and Greenwich, which is 25241,9 toises, gives by the mean of the three assumed ellipticities 26′ 32″,3 for the difference of latitude. After deducting this quantity from 51° 28′ 40″, the supposed latitude of Greenwich, there remains 51° 2′ 7″,7 or 8″, for that of the tower at Dunkirk. If from this again we deduct the calculated arc 2° 11′ 23″,5, we have 48° 50′ 44″,5 for the latitude of the Pantheon, while, according to the observations of M. Delambre, it is 49″,37, or 48″,35 by those of M. Mechain. If various circumstances, with regard to unfavourable weather, and also others of a different kind connected with the revolution, and of which M. Delambre complains with much reason, have occasioned some uncertainty with respect to the observations at Dunkirk, still the numerous observations made at Paris, both by him and by M. Mechain, at a more favourable season, and in times of perfect tranquillity, render the supposition of an error of four seconds in the latitude of the Pantheon wholly inadmissible. It is however too true, that such errors are possible, and it is only by careful perseverance, and by repeated verification, that they are to be discovered and removed, as we have seen to be highly probable with respect to the station at Arbury Hill.

But the same celebrated observer, M. Mechain, who handled instruments with great delicacy, and was possessed of peculiar talents for this species of observation, has given us an instance of singular irregularity in the observations made at Montjui and at Barcelona.

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The latitude of Montjui, determined by a very long and regular series of zenith distances, is full 3″,24 less than that deduced from a similar series of observations made at Barcelona, with the very same instruments, and with equal care. Moreover, there is reason to think, from other observations, that the latitude of Barcelona (which is supposed to be 45″) ought to be diminished still one second, so that the difference between the observations at Montjui and at Barcelona will probably amount to as much as 4″. Local attractions are supposed to have been the cause of this irregularity; but then the latitude, as deduced from observations made at Barcelona, should have been less than it appeared by those made at Montjui itself; for the deviation of the plumb-line (or of the spirit contained in a level) *could only* be occasioned by the little chain of land elevated to 120 or 130 toises, which passes to the north of Barcelona in a north-easterly direction. Now since the deviations arising from this source would be northward, the zenith distance of circumpolar stars would be augmented by that deviation, and consequently the latitude deduced therefrom would be diminished by just so much. But here the contrary occurs; for the latitude of Montjui deduced from the observations at Barcelona is 48″,23, whilst that obtained by direct observations at Montjui is only 45″. Hence it seems probable, that the cause of this irregularity must be sought elsewhere, and that it is not likely to be discovered without repeating over again the same observations.

Moreover it does not follow that the latitudes of two places are correct, because the declinations of the stars deduced from them correspond; for the deviations caused by local attractions, or from any other source, are made to disappear in correcting the declination, but remain uncorrected in the latitude of each.

Lieut. Col. Mudge is also of opinion, that the irregularity in the value of his degree may be ascribed to deviation of the plumb-line, occasioned by local attractions. This is certainly very possible, and may be decided by an examination of all circumstances on the spot. But if there be really an error of 1″ in the extent of the whole arc, this should rather be ascribed to some defect in the observations themselves, than to any extraneous source; for the observations of different stars give results that differ more than four seconds from each other.

I shall now conclude this memoir, by expressing a wish, which men of science in England have it more in their power

power than any others to gratify; I mean by making new measurements in the southern hemisphere. Those which have been made hitherto in the northern hemisphere are extremely satisfactory by their agreement, and give us great reason to presume that the general level of the earth's surface is elliptical, and very regularly so; and hence we might expect the opposite hemisphere to be equally so, and to be a portion of the same curve. Nevertheless the degree measured at the Cape of Good Hope by Lacaille, in latitude 33° 18′, appears to indicate an ellipse of less eccentricity, or of greater axis; for the linear extent of 57037 toises corresponds to the measure of a degree in latitude 47° 47′ in the northern hemisphere. If now we calculate the arc as before, with an oblateness of $\frac{1}{320}$, and with the sides of Lacaille's triangles reduced to the meridian, we find it greater by 10″ than it was found to be by observations of the stars. An error of ten seconds, by an astronomer so skilful and scrupulous as Lacaille, is too extraordinary to be admitted as probable. It is true, that there was a greater error well ascertained to have occurred in the measurement in Lapland, amounting to thirteen seconds; but the academicians engaged in this undertaking were by no means equally conversant with observations as Lacaille.

There remains therefore but one method of removing all doubt on this subject, and this is to repeat and verify the measurement at the Cape, and, if possible, to extend it still further to the north. The same Major Lambton who has succeeded so well in Asia, and is in possession of such perfect instruments for the purpose, would be singularly qualified for a similar undertaking in Africa, and would furnish us with a measurement in the other hemisphere, as much to be relied upon as the former. He would have the glory of deciding two important questions by his own observations: first, the similarity and magnitude of the two hemispheres; and, secondly, the degree of reliance to be placed on the elliptic hypothesis.

It might be still further desirable, if other measurements could also be undertaken, either in New Holland, or in Brazil; for though neither of these countries differs much in latitude from the Cape of Good Hope, they are so remote in longitude, that a correspondence of measures so taken would nearly establish the similarity of all meridians.

Note.

I shall now explain the formulæ employed in deducing

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the results to which I have come in the foregoing Memoir. The demonstration of them is to be found in the work of M. Delambre on the Meridian.

In the first place, let a be the radius of the equator, e the eccentricity, ψ the latitude of one extremity of a side, or arc, in any series of triangles, and θ the azimuth of that side. The radius of curvature of this arc will be expressed by

$$\frac{1}{R1} = \frac{\left(1 + \frac{e^2}{1-e^2} . \cos.^2\psi . \cos.^2\theta\right)}{R} \text{ and } \frac{1}{R} = \frac{(1-e^2 . \sin.^2\psi)^{\frac{1}{2}}}{a}.$$

Hence we see that R is the radius of the arc at right angles to the meridian. One may in general neglect the azimuth, and take the last radius for the radius R1. Now, in computing the arc between Clifton and Dunnose, I have supposed the oblateness to be $\frac{1}{330}$ or $e^2 = \frac{669}{330^2}$, and log. $a =$ 6,5147200 expressed in toises.

The latitude of the southern extremity of the base is the same as that of Clifton, and its azimuth, if we choose to attend to it, is nearly 335° 23′. This base, considered as an arc of a circle, is reduced to its sine by the formula $\varepsilon = \log. \varepsilon - \frac{K . \varepsilon^2}{6R^2}$, (K being the modules of the table of logarithms, so that log. K=9,6377843.)

By means of the logarithmic sine of the base, and the angles of the triangles, considered as spherical, the logarithmic sines of the sides in the series were next computed, and then reduced to logarithms of the arcs themselves by the formula log. $\varepsilon = \log. \sin. \varepsilon + \frac{K . \sin.^2\varepsilon}{6R^2}$.

For the purpose of making this last reduction, it is sufficient to take a single value of R, corresponding to the mean latitude of the entire arc 52° 2′ 20″. It was thus that the table was formed of logarithmic sides considered as arcs.

Let m be one of these arcs, and let us represent by $\delta\psi$ and $\delta\psi''$ its value reduced to the meridian, the one in toises, the other in seconds of a degree, and we shall have the following formulæ:

$$\delta\psi = m . \cos.\theta - \left(\frac{m^2 . \sin.^2\theta}{2R}\right) . \tang.\psi - \left(\frac{m^2 . \sin.^2\theta}{2R}\right) . \left(\frac{m . \cos.\theta}{3R}\right) . (1 + 3 \cdot \tan.^2\psi)$$

$$\delta\psi'' = \left(\frac{\delta\psi}{R . \sin. 1''}\right) + \left(\frac{\delta\psi}{R . \sin. 1''}\right) . e^2 . (1+e^2) . \cos.^2\psi . \left\{1 \mp \left(\frac{3 \tan.\psi}{2}\right) . \left(\frac{\delta\psi}{R}\right)\right\}:$$ the superior sign being taken when

the

the latitude ψ'' is greater than ψ, and the inferior when it is less.

The correction dependent on the convergence of the meridian for the azimuths is $\delta\theta = \left(\frac{m\,.\,\sin.\,\theta}{R1\,.\,\sin.\,1''}\right).\left(\frac{\sin.^2\,(\psi+\psi)}{\cos.\psi'.\cos.\frac{1}{2}\delta\psi''}\right)$.

Hence the azimuth of the first station seen from the second and reckoned westward from the north, is $\theta' = 180^\circ + \theta + \delta\theta$.

If P″ be put for the difference of longitude between two points distant by an arc which measures m, we have sin. $P'' = \frac{\sin.\,m\,.\,\sin.\,\theta}{\cos.\,\psi}$, log. sin. $m = \log.\left(\frac{m}{R1}\right) - \frac{K}{6}.\left(\frac{m}{R1}\right)^2$, and log. $P'' = \log.\left(\frac{\sin.\,P''}{\sin.\,1''}\right) + \frac{6}{K}.(\sin.\,P'')$.

The arc of the meridian, between Greenwich and Formentera, is so fortunately situated, that its middle point is in latitude 45°. Its whole extent measures 12° 48′ 44″, and the distance between the parallels, in linear measure, was found to be 730430,7 toises. Hence the mean degree, corresponding to the latitude of 45° 4′ 18″, is 57010,5 toises; and if we multiply this number by 90°, we get one-fourth part of the meridian of the earth.

The correction to be deduced for oblateness is 58, 59, or 61 toises, according as it is assumed to be $\frac{1}{330}$, $\frac{1}{320}$, or $\frac{1}{310}$, and if we take the mean of these, we have the fourth part of the meridian Q = 5130886 toises; and hence the metre = 44330867 lines; so that the value of the metre turns out to be almost entirely independent of the elliptical form of the earth.

The radius of the equator is derived from the expression $\log.\,a = \log.\left(\frac{2Q}{\pi}\right) + K.\left(\frac{1}{2}.\varepsilon + \frac{1}{16}.\varepsilon^2 - \frac{1}{48}.\varepsilon^3\right)$, ε being the oblateness, and π the periphery of a circle = 3,1416.

In order to compare any degrees measured with those obtained on the elliptic hypothesis, we have a very simple formula. Let m and m' be the values of two degrees on the meridian, of which the mean latitudes are $\psi 1$ and $\psi 2$; in comparing the analytic expressions for these two degrees, developing them, and then making $\psi = 45^\circ$, we have $m' = m.\left(1 - \frac{1}{2}.p.\cos.\,2\psi 2 + g.\cos.^2 2\psi 2\right)$, $m = 57010,5$ toises, $p = \frac{3}{2}e^2.\left(1 + \frac{1}{2}e^2\right).\frac{\sin.\,1^\circ}{1^\circ\,\sin.\,1''}$, and $g = \frac{15}{64}.e^4.\left(\frac{\sin.\,2^\circ}{1^\circ.\sin.1''}\right)$.

And then we shall find that the oblateness $\frac{1}{320}$ gives 57075,66 and 57192,38 toises for the degrees in England and Lapland.

I shall here subjoin one reflection more, which appears of importance. The oblateness of the earth is a quantity which varies considerably, by the least difference in the elements on which it depends. Accordingly it is not surprising, that its value fluctuates between two proportions which differ sensibly from each other. To illustrate this, let p be the function which serves to determine the oblateness of the earth, so that $\frac{1}{1} = p$. When this equation varies $-\delta\varepsilon = \varepsilon^2.\delta p$.

Now the coefficient ε^2 being very great, we see why the least variation in the elements of the function p occasions so considerable a variation in the denominator of the oblateness. This is precisely what happens in the lunar equations dependent on the figure of the earth, and which M. Laplace has deduced from his beautiful theory. Thus, for example, in the inequality that depends on the longitude of the moon's node, which he has determined analytically with so much precision, the numerical coefficient found by Burg gives $\frac{1}{305}$ for the oblateness; but if this coefficient be diminished by 0″,665, then the oblateness becomes $\frac{1}{320}$, so that a variation even to this small amount in the coefficient augments the denominator of the oblateness nearly $\frac{1}{20}$ part.

The same happens with regard to the pendulum vibrating seconds; for, supposing its length at 45° to have been correctly ascertained by MM. Biot and Mathieu, if we wish to know the length of a second's pendulum at the equator, corresponding to an oblateness of $\frac{1}{320}$, we find it to be 439,1810 lines. Now this length differs from that determined by Bouguer only by 0,029 of a line, and M. Laplace even thinks that the result of Bouguer should be diminished by about double this quantity. We see from hence how much these little differences, whether produced by errors of observation, or irregularities in the earth itself, are liable to affect the denominator of the fraction expressing the oblateness.

Fortunately, it seems probable, that the utmost latitude of our present uncertainty is between the limits of 330 and 310, and the mean of these may be considered as a very near approximation to the truth.

XV. *On*

XV. *On the Formation of Sulphur in India.* By Benjamin Heyne, *M.D. Botanist and Naturalist to the Hon. East India Company, and Surgeon in the Madras Army*.*

Sulphur has been considered to be *indigenous* only where deep seated mines of metals are found, or where volcanoes or earthquakes have ravaged the bowels and surface of a country. Nothing therefore is known of its formation, nor have analytical experiments afforded any other than distant hints, and these so very indistinct that our modern chemists have ranked it among simple substances.

Circumstances requisite for the production of any particular substance sometimes, however, unite at accessible places, and it then becomes possible for an attentive observer to penetrate into such mysteries, and to develop them where or when least expected. I will not say that this is precisely the case here, but I trust that what I have observed on this subject will not be thought altogether unworthy of notice.

I must premise, that I have no where found brimstone on the peninsula of India, though always travelling and inquiring into subjects of natural production and curiosity; nor has it been discovered, as far as I know, by any other person, either in a simple state or in combination. Once indeed I understand, from very respectable authority, that a large lump of very fine brimstone was found at Condapitty in the Masulipatam circar, in the trunk of a Margosa tree, (*Melia azedarachta*) torn up, and (as was supposed) shattered to pieces by lightning; I was therefore not a little astonished when a substance in powder or small pieces evidently brimstone was shown me in the Northern circars, with the intimation that it had been collected on the banks of the Godavery.

The place to which I was directed is not far from Maddepollam and Ammalapore, places situated about half way between Coringa and Masulipatam, and between the branches of the river Godavery, known for the manufacture of fine long cloth, which is carried on to a great extent in this part of the country; but, even there, this circumstance was unknown to all with whom I conversed. My guide however convinced me soon of the truth of this assertion, by conducting me to a small village about twelve miles east of Ammalapore, called Soora-Sauny-Yanam, belonging to the Bommadauram Mootak, one of the Ped-

* Communicated by the author.

 datore

datore rajah's districts. Hard by is a lake in which I found the confirmation of my researches. It is a narrow lake extending several miles in the direction from south to north along the village, and seems to be every where very shallow. At its southern extremity it communicates with a branch of the Godavery and a salt-water creek, from which it receives its water in the rainy monsoon.

In the hot season it is nearly dry, and the mud then exposed to the sun exhales a disagreeable smell, which at some places I thought was like that of a sulphuret.

The first excursion I made with my guides was to a place due west of the village, where they went trampling up and down in the water, and at times taking up a handful of mud, which, on examination, certainly had a faint smell of brimstone, but did not at all resemble the substance which had been shown to me some time ago, and which had induced me to make this expensive excursion.

Under the full impression of disappointment, I was sitting after my fruitless return to the village in my palanquin, scarcely observing that it was surrounded by a number of inquisitive visitors, when on a sudden my attention was attracted by the clamorous vociferations of a woman in the pursuit of all my palanquin bearers, who had robbed her little garden of a pumpkin. She appealed to the renter for protection; but he, like many in his situation in absolute power, magnanimously made a present of it to the strangers, who were carrying their booty off in great triumph. Unluckily for them, however, I interfered, and ordered them to restore the stolen goods, which brought on a slight but friendly altercation between me and the renter; and this ended in the payment for the pumpkin, and an offer of all the bystanders to conduct me to the place from which they collected brimstone.

I then followed a man whom they procured, immediately to the northern extremity of the lake, where we found without much searching brimstone in small heaps and in abundance.

I was told that this substance was to be found further north in the same lake, and in small quantities only to the southward, where the lake gets soonest dry. There it is collected in a loose soft form, or in semi-indurated nodules of a grayish yellow colour after it is dry; and never deeper than a foot from the very surface of the ground on which the water stands.

This salt lake, I learnt, was but of recent formation. Only fifty years ago, the spot where it is now found was under

under cultivation. The country for a great number of miles in all directions is quite plain; nay, I may add that not a hillock is to be seen within fifty miles.

Stones of all kinds are nearly as scarce, except some indurated marl which I found in the stratum below the superficial one.

The soil all over this part of the country is either a rich red earth mixed with vegetable mould, which renders it very productive; or it is the black vegetable cotton soil, which is always accompanied with a stratum of marl. This is also the soil which I observed on the spot where the lake is.

Earthquakes are entirely unknown here, and volcanic substances are not to be found.

It might be supposed that the brimstone found here was deposited by the water of the Godavery, as the lake is in conjunction with one of its smaller branches; or that it had been thrown up from the sea, with which it is also connected. Against the former supposition may be adduced, that it is found in none of the manifold beds of that river, or in its vicinity; and against the second, that it is not observed in any other creek or inlet, and here only where it is remotest from the sea.

Against the existence of extinguished volcanoes, or earthquakes, may I think most strongly be urged the confined compass of the spot where this substance is found; besides what has been observed before of the appearance of the country in general, and its minerals. The only way to account for its existence in the humid way therefore is, in my opinion, the supposition of its having been formed here. The substances we have then to consider are sea water, lime, and vegetable mould.

I filled some bottles with the water of this lake, and having carried them along with me for further examination, I found that neither the nitric nor sulphuric acids had any visible effect on it.

Soda precipitated immediately a plentiful white sediment. Oxalic acid produced a copious sediment. Muriate of barytes caused also a plentiful precipitate.

All I wished to ascertain was, whether this water contained alkaline or calcareous sulphurets, or the sulphuric acid in a free state.

From the few experiments above noted, it appears however that it is not impregnated with sulphurets of any description, as these would have been precipitated both by the sulphuric and nitric acids; but that, like most sea waters,

 it

it contains some sulphates; and probably the sulphate of lime, as the latter basis was indicated by the oxalic acid, and the former by the sulphuric acid and the muriate of barytes. I will not enter upon any theoretical disquisitions; but I cannot help observing, that the presence of brimstone in substances which not only can but actually do produce hydrogen gas in such abundance, has suggested to my mind that sulphur itself may be a product of them, and possibly only a modification of hydrogen.

XVI. *Of such Portions of a Sphere as have their Attraction expressed by an algebraic Quantity.*

(Concluded from vol. xl. p. 329.)

Sir, I assigned, in a former letter, such cylindric portions as when taken from a sphere, or hemisphere, will leave a remainder having an algebraic quantity for the measure of its attraction. There is yet another problem; viz. to find the nature of the curve bounding the base, when the attraction of the cylindric portion *itself* is an algebraic quantity. It is scarcely necessary to observe that equation (β), which supposes the fluent, with respect to θ, to be taken from $\theta=0$, to $\theta=\frac{\pi}{2}$, is only adapted to particular cases. Let us take the general form:

$$F=\frac{4}{3}\int\left\{\frac{(2R\cos.\theta)^{\frac{3}{2}}}{(2R\cos.\theta)^{\frac{1}{2}}}-\frac{(2R\cos.\theta-r)^{\frac{3}{2}}}{(2R\cos.\theta)^{\frac{1}{2}}}\right\}\ \text{Cos}.\,\theta.\dot{\theta}\ldots\ldots(\gamma);$$

Or,

$$F=\frac{4}{3}R\int\dot{\theta}+\frac{4}{3}R\int\text{Cos}.\,2\theta.\dot{\theta}-\frac{4}{3}\int\frac{(2R\cos.\theta-r)^{\frac{3}{2}}}{(2R\cos.\theta)^{\frac{1}{2}}}\text{Cos}.\theta.\dot{\theta}\ (\delta).$$

A simple inspection of these forms will point out many ways of effecting what we propose.

Make, in equation (γ), $r=2R\cos.\theta\left\{1-(1-\cos.^{n}\theta)^{\frac{2}{3}}\right\}$ and it becomes

$$F=\frac{4}{3}\int\left\{\frac{(2R\cos.\theta)^{\frac{3}{2}}}{(2R\cos.\theta)^{\frac{1}{2}}}-\frac{(2R\cos.\theta)^{\frac{3}{2}}}{(2R\cos.\theta)^{\frac{1}{2}}}(1-\cos.^{n}\theta)\right\}\text{Cos}.\,\theta.\dot{\theta}=$$

$\frac{8}{3}R\int\text{Cos}.^{n+2}\theta.\dot{\theta}$; the integral to be taken from $\theta=0$, to $\theta=\frac{\pi}{2}$. It is evident, that this will be an algebraic quantity, as the problem requires, when n is any odd whole positive number. The curve, moreover, which bounds the base of the cylinder is always algebraic.

Defi-

Definition.

Let (A) and (B) be portions, intercepted within the hemisphere (H), of different cylinders; if the attraction of (A) be equal to the attraction of (H)$-$(B), I call these cylinders *reciprocal* as to attraction.

Problem.

To find any number of reciprocal cylinders such that the attraction of (A), *or its equal* (H)$-$(B), *shall be an algebraic expression.*

This will be effected, if the curves bounding the bases of (A) and (B) be of such a nature, that the radii vectores, drawn from the attracted point, are,

$$\text{for (A)},\ r = 2\mathrm{R}\cos.\theta\left\{1-(1-\cos.^{3n}\theta)^{\frac{2}{3}}\right\}$$

$$\text{for (B)},\ r' = 2\mathrm{R}\cos.\theta\,(1-\cos.^{2n}\theta),$$

and n be taken any odd whole positive number: for, by what has been shown, in this and the former paper, we have

Attraction of (A) = Attraction of (H) $-$ (B) = $\frac{8}{3}\mathrm{R}\int\cos.^{3n+2}\theta\,.\,\dot{\theta}$ the fluent to be taken from $\theta = 0$, to $\theta = \frac{\pi}{2}$.*

We may find cylinders whose portions, intercepted by the hemisphere, have algebraic expressions for their attraction, by making $r = 2\mathrm{R}\cos.\theta\,(1-m\cos.^{2n}\theta)$, or $r = 2\mathrm{R}\cos.\theta\,(1-m\sin.^{2n}\theta)$, and determining m in such a manner as to eliminate the arcs from the expression of the attraction. But this will not be so neat as the former method, because there will be radicals employed. I shall however give an example:

Substitute, in (δ), the first of the above-mentioned values, and there arises

$$\mathrm{F} = \frac{4}{3}\mathrm{R}\int\dot{\theta} + \frac{4}{3}\mathrm{R}\int\cos.2\theta\,.\,\dot{\theta} - \frac{8}{3}m^{\frac{3}{2}}\mathrm{R}\int\cos.^{3n+2}\theta\,.\,\dot{\theta};$$

here $3n+2$ must be a whole even positive number, greater than 2: as to the integral, it must evidently be taken from such a value of θ, as gives $\cos.^{2n}\theta = \frac{1}{m}$, to $\theta = \frac{\pi}{2}$. For a particular example, let $3n+2=4$, or $n = \frac{2}{3}$; then,

* Those cylinders will also be reciprocal, the equations of whose bases are $r = 2\mathrm{R}\cos.\theta\left\{1-(1-\sin.^{3n}\theta)^{\frac{2}{3}}\right\}$, $r' = 2\mathrm{R}\cos.\theta\,(1-\sin.^{2n}\theta)$.

$\frac{8}{3} m^{\frac{3}{2}} R \int \cos.^{3n+2} \theta . \dot{\theta} = \frac{m^{\frac{3}{2}} R}{3} \int 8 \cos.^{4} \theta . \dot{\theta} = \frac{m^{\frac{3}{2}} R}{3} \int \Big\{ 3\dot{\theta} + 4 \cos. 2\theta . \dot{\theta} + \cos. 4\theta . \dot{\theta} \Big\}$, and $F = \left(\frac{4}{3} - m^{\frac{3}{2}}\right) R \int \dot{\theta} + (1 - m^{\frac{3}{2}}) \frac{4}{3} R \int \cos. 2\theta . \dot{\theta} - \frac{m^{\frac{3}{2}} R}{3} \int \cos. 4\theta . \dot{\theta}$.

Here the arcs are avoided by making $m^{3} = \frac{4}{3}$, or $m = \left(\frac{4}{3}\right)^{\frac{2}{3}}$; whence, $F = -\frac{4}{9} R \int \Big\{ \cos. 2\theta . \dot{\theta} + \cos. 4\theta . \dot{\theta} \Big\} = -\frac{2}{9} R \sin. 2\theta - \frac{1}{9} R \sin. 4\theta$.

This is the attraction of the portion of such a cylinder as has, for the radius vector of its base, $r = 2R \cos. \theta \left(1 - \left(\frac{4}{3}\right)^{\frac{2}{3}} \cos.^{\frac{4}{3}} \theta\right)$. The base of the cylinder will plainly consist of two parts like fig. 3 of the former paper.

The equation $\left(\frac{4}{3}\right)^{\frac{2}{3}} \cos.^{\frac{4}{3}} \theta = 1$ gives $\cos. \theta = \frac{\sqrt{3}}{2}$; so that each portion of the base lies between 30 and 90 degrees, on each side of the diameter, passing through the attracted point; and, within these limits,

$$F = \frac{2}{9} R \times \frac{\sqrt{3}}{2} + \frac{1}{9} R \times \frac{\sqrt{3}}{2} = \frac{3}{9} R \frac{\sqrt{3}}{2} = \frac{R}{2\sqrt{3}}.$$

Having terminated what I meant to say respecting the *attraction* of this kind of solids, I will add a word or two concerning their solidity.

Let fig. 4 (Plate III) represent the base of a hemisphere, A its centre, ABCG a curve, whose parts, on each side of the radius AC, are equal and similar. Put $R = AC$, $r = AD$, $\theta =$ the angle DAC. If we conceive a cylinder erected on the base ABCG, the solidity (S) of the part intercepted within the hemisphere is evidently $S = 2\iint \sqrt{R^2 - r^2}\, r \dot{r} \dot{\theta}$; or, taking the fluent, with respect to r, so that it may vanish when $r = 0$,

$$S = \frac{2}{3} R^3 \theta - \frac{2}{3} \int (R^2 - r^2)^{\frac{3}{2}} \dot{\theta} \ldots\ldots\ldots (\varepsilon).$$

Make, in (ε), $r^2 = R^2 (1 - \sin.^{2n} \theta)$; and, because the curve denoted by this equation is contained in half the base of the hemisphere, it becomes

$$S = \frac{\pi}{3} R^3 - \frac{2}{3} R^3 \int \sin.^{3n} \theta . \dot{\theta}.$$

Now, the first term $\frac{\pi}{3} R^3$ is the solidity of one half

of

of the hemisphere; consequently, the other part, viz. $\frac{2}{3}R^3\int \sin.^{3n}\theta . \dot{\theta}$ is what remains of the half hemisphere after taking away the included portion of the cylinder, whose base is defined by the equation $r^2 = R^2(1 - \sin.^{2n}\theta)$: and this expression of the solidity will always be algebraic, when n is an odd whole positive number. Let us now find the equation of these curves in rectangular coordinates. Put $x = An$, $y = mn$; then $r^2 = R^2(1 - \sin.^{2n}\theta)$ becomes $x^2 + y^2 = R^2\left(1 - \frac{y^{2n}}{(x^2+y^2)^n}\right)$; or, $(x^2 + y^2)^{n+1} = R^2\left\{(x^2+y^2)^n - y^{2n}\right\}$, where n is an odd positive whole number. When $n = 1$, $x^2 + y^2 = Rx$, and the curve is a circle: This is *the well-known case of Bossut.*

PROBLEM.

It is required to assign the bases of cylinders, which may be the *reciprocals* (as to solidity) of those already found: that is, whose portions, included within the half hemisphere, may $= \frac{2}{3}R^3\int \sin.^{3n}\theta . \dot{\theta}$.

This will be effected if we put, in equation (ε), $r^2 = R^2\left\{1 - (1 - \sin.^{3n}\theta)^{\frac{2}{3}}\right\}$, for there results $S = \frac{2}{3}R^3\theta - \frac{2}{3}R^3\int(1 - \sin.^{3n}\theta).\dot{\theta} = \frac{2}{3}R^3\int \sin.^{3n}\theta . \dot{\theta}$, the fluent to be taken from $\theta = 0$ to $\theta = \frac{\pi}{2}$. If we want these cylinders to have their included portions algebraic, n must be an odd whole positive number.

I add another

PROBLEM.

Assign the base of such a cylinder, as shall have an algebraic expression for the solidity of that portion which is intercepted in the half hemisphere; and shall satisfy the further condition, that this intercepted solidity shall approach as near as we please to that of the half hemisphere itself.

Make, in equation (ε), $r^2 = R^2\left\{1 - (1 - \cos. n\theta)^{\frac{2}{3}}\right\}$, it becomes

$$S = \frac{2}{3}R^3\theta - \frac{2}{3}R^3\int(1 - \cos. n\theta)\,\dot{\theta} = \frac{2}{3}R^3\int \cos. n\theta . \dot{\theta} =$$

$\frac{2R^3}{3n}$

$\frac{2R^3}{3n}$ sin. $n\theta$. Now, let $n = \frac{1}{2^m}$, m being a large positive whole number; the cos. $n\theta$ will differ very little from unit, or r will very nearly equal R between the limits $\theta = 0$, and $\theta = \frac{\pi}{2}$, within which limits $S = \frac{2^{m+1}}{3} \times R^3 \times \sin. \frac{90^\circ}{2^m}$ which is algebraic, because $\sin. \frac{90^\circ}{2^m}$ may be expressed algebraically; and this value of S very nearly equals $\frac{\pi}{3} R^3$, as the problem requires.

I am, sir,

Your obedient servant,

X. Y.

XVII. *Of Coffee, and the Art of preparing it. Extracted from Count* RUMFORD'S *Eighteenth Essay.*

THE author remarks that, "among the numerous luxuries of the table, unknown to our forefathers, coffee may be considered as one of the most valuable. Its taste is very agreeable, and its flavour uncommonly so; but its principal excellence depends on its salubrity, and on its exhilarating quality. It excites cheerfulness, without intoxication; and the pleasing flow of spirits which it occasions, lasts many hours, and is never followed by sadness, languor, or debility. It diffuses over the whole frame a glow of health, and a sense of ease and well being which is extremely delightful: existence is felt to be a positive enjoyment, and the mental powers are awakened, and rendered uncommonly active." After some other judicious observations on the valuable properties of coffee, and the uncertainty of the result in the common methods of preparing it, the Count proceeds with his subject.

"Different methods have been employed in making coffee; but the preparation of the grain is nearly the same in all of them. It is first roasted in an iron pan, or in a hollow cylinder made of sheet-iron, over a brisk fire; and when, from the colour of the grain, and the peculiar fragrance which it acquires in this process, it is judged to be sufficiently roasted, it is taken from the fire, and suffered to cool. When cold it is pounded in a mortar; or ground in a hand-mill to a coarse powder, and preserved for use.

"Great care must be taken in roasting coffee, not to roast it too much: as soon as it has acquired a deep cinnamon

namon colour, it should be taken from the fire, and cooled; otherwise much of its aromatic flavour will be dissipated, and its taste will become disagreeably bitter.

"In some parts of Italy, coffee is roasted in a thin Florence flask slightly closed by means of a loose cork. This is held over a clear fire of burning coals, and continually agitated. As no visible vapour ever makes its appearance within the flask, the colour of the coffee may be distinctly seen through the glass, and the proper moment seized for removing the coffee from the fire.

"I have endeavoured to improve this Italian method, by using a thin globular glass vessel with a long narrow cylindrical neck. This globular vessel is six inches in diameter, and its cylindrical neck is one inch in diameter and 18 inches long. It is laid down horizontally, and supported in such a manner on a wooden stand as to be easily turned round its axis. The globular vessel projects beyond the stand, and is placed, at a proper height, immediately over a chafing-dish of live coals. When this globular vessel is blown sufficiently thin; and when care is taken to keep it constantly turning round, when it is over the fire, there is not the smallest danger of its being injured by the heat, however near it may be to the burning coals.

"In order that coffee may be perfectly good, and very high flavoured, not more than half a pound of the grain should be roasted at once; for, when the quantity is greater, it becomes impossible to regulate the heat in such a manner as to be quite certain of a good result.

"The end of the cylindrical neck of the globular vessel should be closed by a fit cork, having a small slit in one side of it to permit the escape of the vapour out of the vessel. This cork should project about an inch beyond the extremity of the neck of the vessel, in order that it may be used as a handle in turning the vessel round its axis, towards the end of the process, when the neck of the vessel becomes very hot. The progress of the operation, and the moment most proper to put an end to it, may be judged and determined with great certainty, not only by the changes which take place in the colour of the grain, but also by the peculiar fragrance which will first begin to be diffused by it when it is nearly roasted enough. This fragrance is certainly owing to the escape of a volatile, aromatic substance, which did not originally exist, *as such*, in the grain, but which is formed in the process of roasting it. By keeping the neck of the globular vessel cold, by means of wet cloths, I found means to condense this

this aromatic substance, together with a large portion of aqueous vapour with which it was mixed.

"The liquor which resulted from this condensation, which had an acid taste, was very high flavoured, and as colourless as the purest water; but it stained the skin of a deep yellow colour, which could not be removed by washing with soap and water; and this stain retained a strong smell of coffee several days.

"I have made several unsuccessful attempts to preserve the fragrant aromatic matter which escapes from coffee when it is roasting, by transferring it to other substances. Perhaps others may be more fortunate. But I must not suffer myself to be enticed away from my subject by these interesting speculations.

"If the coffee in powder is not well defended from the air, it soon loses its flavour, and becomes of little value; and the liquor is never in so high perfection as when the coffee is made immediately after the grain has been roasted.

"This is a fact well known to those who are accustomed to drinking coffee, in countries where the use of it is not controled by the laws; and if a government is seriously disposed to encourage the general use of coffee, individuals must be permitted to roast it in their own houses.

"As the roasting and grinding of coffee take up some considerable time, and cannot always be done without inconvenience at the moment when the coffee is wanted; I contrived a box for keeping the ground coffee, which I have found, by several years' experience, to preserve the coffee much better than any of the vessels commonly used for that purpose. It is a cylindrical box made of strong tin, four inches and a quarter in diameter, and five inches in height, formed as accurately as possible within, to which a piston is so adapted as to close it very exactly; and, when pressed down into it, to remain in the place where it is left, without being in danger of being pushed upwards by the elasticity of the ground coffee which it is destined to confine.

"This piston is composed of a circular plate of very stout tin, which is soldered to the lower part of an elastic hoop of tin, about two inches wide, which is made to fit into the cylindrical box as exactly as possible, and so as not to be moved up and down in it without employing a considerable force. This hoop is rendered elastic, by means of a number of vertical slits made in the sides of it.

On the upper side of the circular plate of tin, which closes this hoop below, and in the centre of it, there is fixed a strong

a strong ring, of about one inch in diameter, which serves instead of a piston rod, or a handle for the piston. The cylindrical box is closed above by a cover, which is fitted to it with care, in order that the air which is shut up within the box (between the piston and the cover) might be well confined."——

"Boiling hot water extracts from coffee, which has been properly roasted and ground, an aromatic substance of an exquisite flavour. together with a considerable quantity of astringent matter, of a bitter but very agreeable taste; but this aromatic substance, which is supposed to be an oil, is extremely volatile, and is so feebly united to the water that it escapes from it into the air with great facility. If a cup of the very best coffee, prepared in the highest perfection, and boiling hot, be placed on a table, in the middle of a large room, and suffered to cool, it will in cooling fill the room with its fragrance; but the coffee, after having become cold, will be found to have lost a great deal of its flavour. If it be again heated, its taste and flavour will be still further impaired; and after it has been heated and cooled two or three times, it will be found to be quite vapid and disgusting. The fragrance diffused through the air is a sure indication that the coffee has lost some of its most volatile parts; and as that liquor is found to have lost its peculiar flavour, and also *its exhilarating quality*, there can be no doubt but that both these depend on the preservation of those volatile particles which escape into the air with such facility."——

"In order that coffee may retain all those aromatic particles which give to that beverage its excellent qualities, nothing more is necessary than to prevent all internal motions among the particles of that liquid; by preventing its being exposed to any change of temperature, either during the time employed in preparing it; or afterwards, till it is served up.

"This may be done by pouring boiling water on the coffee in powder; and surrounding the machine in which the coffee is made, by boiling water; or by the steam of boiling water: for the temperature of boiling water is *invariable*, (while the pressure of the atmosphere remains the same,) and the temperature of steam is the same as that of the boiling water from which it escapes.

"But the temperature of boiling water is preferable to all others for making coffee, not only on account of its *constancy*, but also on account of its being most favourable to the extraction of all that is valuable in the

the roasted grain. I found that coffee infused with boiling water was always higher flavoured, and better tasted, than when the water used in that process was at a lower temperature."—

"As all kinds of agitation must be very detrimental to coffee, not only when made, but also while it is making, it is evident that the method formerly practised, that of putting the ground coffee into a coffee-pot with water, and boiling them together, must be very defective, and must occasion a very great loss. But that is not all; for the coffee which is prepared in that manner can never be good, whatever may be the quantity of ground coffee that is employed. The liquor may no doubt be very bitter, and it commonly is so; and it may possibly contain something that may irritate the nerves,—but the exquisite flavour and exhilarating qualities of good coffee will be wanting."——

"Coffee may easily be too bitter, but it is impossible that it should ever be too fragrant. The very smell of it is reviving, and has often been found to be useful to sick persons, and especially to those who are afflicted with violent head-achs. In short, every thing proves that the volatile, aromatic matter, whatever it may be, that gives flavour to coffee, is what is most valuable in it, and should be preserved with the greatest care; and that, in estimating the strength or richness of that beverage, its *fragrance* should be much more attended to, than either its bitterness or its astringency."——

"One pound averdupois, of good Mocha coffee, which, when properly roasted and ground, weighs only fourteen ounces, serves for making fifty-six full cups of the very best coffee, in my opinion, that can be made.

"The quantity of ground coffee which I use for one full cup, is 108 grains troy, which is rather less than a quarter of an ounce. This coffee, when made, would fill a coffee-cup of the common size, quite full; but I use a larger cup, into which the coffee being poured boiling hot, on a sufficient quantity of sugar (half an ounce), I pour into it about one-third of its volume of good sweet cream, *quite cold*. On stirring these liquids together, the coffee is *suddenly cooled*, and in such a manner as not to be exposed to the loss of any considerable portion of its aromatic particles in that process.

"In making coffee, several circumstances must be carefully attended to: in the first place, the coffee must be ground fine, otherwise the hot water will not have time to penetrate to the centres of the particles; it will merely soften them at their surfaces, and, passing rapidly between them,

them, will carry away but a small part of those aromatic and astringent substances on which the goodness of the liquor entirely depends. In this case, the grounds of the coffee are more valuable than the insipid wash which has been hurried through them, and afterwards served up under the name of coffee."——

"As a *gill* is a measure well known in England, I shall adopt it as a standard measure for a cup of coffee; and as it is inconvenient to fill coffee-cups quite full to the brim, I shall propose coffee-cups to be made of the form and dimensions they now commonly have, or of a size proper for containing $8\frac{1}{3}$ cubic inches of liquor, when filled quite full to the brim. I have found by the results of a great number of experiments, that *one quarter of an ounce* averdupois of ground coffee is quite sufficient to make a gill of most excellent coffee, of the highest possible flavour, and quite strong enough to be agreeable."——

"Formerly, the ground coffee being put into a coffee-pot, with a sufficient quantity of water, the coffee-pot was put over the fire, and after the water had been made to boil a certain time, the coffee-pot was removed from the fire, and the grounds having had time to settle, or having been fined down with isinglass, the clear liquor was poured off, and immediately served up in cups.

"From the results of several experiments which I made with great care, in order to ascertain what proportion of the aromatic and volatile particles in the coffee escape, and are left in this process, I found reason to conclude, that it amounts to considerably more than half."——

"When coffee is made in the most advantageous manner, the ground coffee is pressed down in a cylindrical vessel, which has its bottom pierced with many small holes, so as to form a strainer; and a proper quantity of boiling hot water being poured cautiously on this layer of coffee in powder, the water penetrates it by degrees, and after a certain time begins to filter through it. This gradual percolation brings continually a succession of fresh particles of pure water into contact with the ground coffee; and when the last portion of the water has passed through it, every thing capable of being dissolved by the water will be found to be so completely washed out of it, that what remains will be of no kind of value.

"It is however necessary to the complete success of this operation, that the coffee should be ground to a powder sufficiently fine."——

"In order that the coffee may be perfectly good, the stratum of ground coffee, on which the boiling water is

poured, must be of a certain thickness, and it must be pressed together with a certain degree of force. If it be too thin, or not sufficiently pressed together, the water will pass through it too rapidly; and if the layer of ground coffee be too thick, or if it be too much pressed together, the water will be too long in passing through it, and the taste of the coffee will be injured."

The author recommends as of importance that the surface of the coffee be rendered quite level after it is put into the strainer, before any attempt is made to press it together, that the water in percolating may act equally on every part. For this purpose he uses the following contrivance: "The circular plate of tin, with a rod fastened to its centre, which serves as a rammer for pressing down the ground coffee, has four small projecting square bars, of about one-tenth of an inch in width, fastened to the under side of it, and extending from the circumference of the plate to within about one quarter of an inch of its centre. On turning this plate round its axis, by means of the rod which serves as a handle to it, (the rod being made to occupy the axis of the cylindrical vessel,) the projecting bars are made to level the ground coffee; and after this has been done, and not before, the coffee is pressed together.

"This circular plate is pierced by a great number of small holes, which permit the water to pass through it, and it remains in the cylindrical vessel during the whole of the time that the coffee is making. It reposes on the surface of the ground coffee, and prevents its being thrown out of its place by the water which is poured on it. The rod which serves as a handle to this circular plate is so short, that it does not prevent the cover of the cylindrical vessel from being put down into its place."

Two-thirds of an inch answers best for the coffee in powder before it is pressed together, and the pressure should be such as to reduce the thickness to something less than *half an inch*.

"A Table, showing the diameters and heights of the cylindrical vessels (or strainers) to be used in making the following quantities of coffee:

Quantity of Coffee to be made at once.	Diameter of the Strainer.	Height of the Strainer.
1 cup	$1\frac{1}{2}$ inches	$5\frac{1}{4}$ inches.
2 cups	$2\frac{1}{8}$	$5\frac{1}{4}$
3 or 4 cups	$2\frac{3}{4}$	5
5 or 6 cups	$3\frac{1}{2}$	$5\frac{1}{8}$
7 or 8 cups	4	$5\frac{1}{4}$
9 or 10 cups	$4\frac{5}{8}$	$5\frac{1}{3}$
11 or 12 cups	5	$5\frac{1}{2}$."

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As these heights are nearly equal, the Count recommends that the strainers be all made of the height of $5\frac{1}{2}$ inches, and suspended in their reservoir at such a height that their bottoms be above the percolated fluid when all has passed through.

"The reservoir and its boiler must be soldered together above, at their brims; and the reservoir must be suspended in its boiler, in such a manner that its bottom may be about a quarter of an inch above the bottom of the boiler.

"The small quantity of water which it will be necessary to put into the boiler, in order that the reservoir for the coffee may be surrounded by steam, may be introduced by means of a small opening on one side of the boiler, situated above, and near the upper part of its handle.

"The spout through which the coffee is poured out passes through the side of the boiler, and is fixed to it by soldering. The cover of the boiler serves at the same time as a cover for the reservoir, and for the cylindrical strainer; and it is made double, in order more effectually to confine the heat.

"The boiler is fixed below to a hoop, made of sheet-brass, which is pierced with many holes. This hoop, which is one inch in width, and which is firmly fixed to the boiler, serves as a foot to it when it is set down on a table, and it supports it in snch a manner that the bottom of the boiler is elevated to the height of half an inch above the table.

"When the boiler is heated over a spirit lamp, or over a small portable furnace in which charcoal is burnt, as the vapour from the fire will pass off through the holes made in the sides of the hoop, the bottom of the hoop will always remain quite clean, and the table-cloth will not be in danger of being soiled when this coffee-pot is set down on the table.

"As the hoop is in contact with the boiler, in which there will always be some water, it will be so cooled by this water as never to become hot enough to burn the table-cloth.

"The bottom of the boiler may be cleaned occasionally, on the underside, with a brush or a towel; but it should not be made bright; for when it is bright it will be more difficult to heat the water in it than when it is tarnished and of a dark-brown colour.

"But the sides of the boiler should be kept as bright as possible; for, when its external surface is kept clean and bright, the boiler will be less cooled by the surrounding

cold bodies, than when its metallic splendour is impaired by neglecting to clean it *.

"As the small quantity of water which is put into the boiler serves merely for generating the steam which is necessary in order to keep the reservoir and its contents constantly boiling-hot; if the reservoir be made of silver, or even of common tin, the boiler may, without the smallest danger, be made of copper; or of copper plated with silver, which will give to the boiler an elegant appearance, and at the same time render it easy to keep it clean on the outside.

"The boiler may likewise be made of tin, and neatly japanned on the outside, provided the hoop to which it is fixed below be made of copper; but this hoop must never be japanned nor painted; and it must always be made of sheet-copper or silver; and the boiler must always be heated over a small portable fire-place or lamp, somewhat less in diameter above, than the hoop on which the boiler is placed.

"In order that the flat bottom of the boiler may not smother and put out the fire, the brim of the small furnace or chafing-dish, which is used, must have six projecting knobs at the upper part of it, each about one quarter of an inch in height, on which the bottom of the boiler may rest.

"If these knobs (which may be the large heads of six nails) be placed at equal distances from each other, the boiler will be well supported; and as the hot vapour from the fire will pass off freely between them, the fire will burn well. As a very small fire is all that can be wanted, no inconvenience whatever will arise from the heating of the boiler on the table, in a dining-room or breakfast-room, especially if a spirit lamp be used; and the quantity of heat wanted is so very small, when the water is put boiling hot into the boiler, that the expense for spirits of wine would

* "I have in my possession two porcelain tea-pots of the same form and dimensions, one of which is gilt all over on the outside, and might easily be mistaken for a gold tea-pot; the other is of its natural white colour, both within and without; being neither painted nor gilt. When they are both filled at the same time with boiling water, and exposed to cool in the same room, that which is gilt retains its heat half as long again as that which is not gilt. The times employed in cooling them a given number of degrees, are as three to two.

"The result of this interesting experiment (which I first made about seven years ago) affords a good and substantial reason for the preference which English ladies have always given to silver tea-pots. The details of this experiment may be seen in a paper published in the Memoirs of the French National Institute for the year 1807."

not,

not, in London, amount to one penny a day, when coffee is made twice a day for four persons.

"It is a curious fact, but it is nevertheless most certain, that, *in some cases*, spirits of wine is cheaper, when employed as fuel, even than wood. With a spirit lamp constructed on Argand's principle, but with a chimney made of thin sheet iron, which I caused to be made about seven years ago, (and which has since become very common in Paris*,) I heated a sufficient quantity of cold water, to make coffee for the breakfast of two persons, and kept the coffee boiling hot, one hour after it was made, with as much spirits of wine as cost *two sous*, or one penny English money."

Description of the Figures.

"*a*—(Fig. 1. Pl. III.) is the cylindrical strainer, into which the ground coffee is put, in order that boiling hot water may be poured on it; when this strainer is filled with boiling water (after ground coffee has been properly pressed down on its bottom.)

"*b*—is the ground coffee in its place.

"*c*—is the handle of the rammer which is represented in its place.

"*d*—is the reservoir for receiving the coffee which descends into it from the strainer; and

"*e*—is the spout through which the coffee is poured out.

"*f*—is the boiler, into which a small quantity of water is put, for the sole purpose of generating steam, for keeping the reservoir hot.

"*g*—is the opening by which the water is poured into the boiler or out of it; this opening has a flat cover, which moves on a hinge, that is represented in the figure.

"The boiler is of a conical form, and is enlarged a little at its upper extremity, in order to receive the cover which closes it above.

"The reservoir and the boiler are fixed together above by soldering, so that the reservoir remains suspended in the boiler.

"The cylindrical strainer is suspended on the upper extremity of the reservoir, by means of a flat projecting brim about two-tenths of an inch broad.

"*h*—is the hoop, made of sheet-copper, and perforated with a row of holes, on which the boiler reposes: a

* "I intend, if possible, to send one of these spirit lamps to England, with this Essay, in order that it may be put into the hands of some workman there, who may be disposed to imitate it."

part of the bottom of the boiler is seen through these holes.

"The reservoir is represented by dotted lines, in order the better to distinguish it.

"The diameter of the hoop *h*, on which the coffee-pot stands, should always be at least *six inches in diameter* whatever may be the contents of the coffee-pot; and the spirit lamps or portable furnaces, used with these coffee-pots, should always be *rather less than six inches in diameter above*, or at their openings, in order that the bottom of the coffee-pot may, in all cases, be set down properly on the six knobs, belonging to the lamp or the furnace, which are destined to support it.

"The figure 2. has been added, in order to show how the same coffee-pot may be made to serve for making any number of cups of coffee, within certain limits, that may be wanted, by being furnished with strainers of different sizes, (*i*, *k*, *l*.)

"Each of these strainers has its separate rammer to ram down the ground coffee placed in it, but one common handle serves for them all. This handle is screwed into the middle of a circular plate, which forms the principal part of the rammer.

"The circular plate which belongs to each of these strainers, remains in it when the coffee-pot is not in use, and the handle remains attached to the circular plate belonging to the smaller strainer."

One or other of these strainers is used in proportion to the number of cups wanted—or they may be used in succession, for any number; and as the heat always remains the same during the whole of the time employed in these operations, the coffee is just as good as if the whole of it were made at once.

In these coffee-pots the boilers may be "made sufficiently capacious for heating the water necessary for making the coffee, as well as that which is required for generating the steam which is employed for keeping the reservoir boiling hot. But when this method is employed, it will be necessary that the boiler should be furnished with a brass cock, placed about one quarter of an inch above the level of its bottom, in order that the boiling water necessary for pouring on the ground coffee in the strainer may be drawn off without removing the boiler from the fire. By placing this brass cock immediately under the handle of the coffee-pot, it may be so united to it as almost to escape observation."——

"As coffee is very wholesome, and may be afforded at a very low

low price, especially in countries which have colonies where the climate is proper for growing it, many public advantages would be derived from the general introduction of it among all classes of society. One most important advantage, which, on a superficial view of the subject, is not very obvious, would most probably be derived from it. As coffee possesses, in a high degree, an exhilarating quality, it would, in some measure, supply the place of spirituous liquors among the lower classes of the people."

Persons who may not find it convenient to use spirit lamps and portable furnaces, may use these coffee-pots over a common chimney fire, in which case the perforated hoops are not necessary.

"For very poor persons, who cannot afford to buy a coffee-pot, I shall recommend a very simple contrivance, by means of which coffee may be made, and even in the highest possible perfection.—I have often made use of this contrivance in preparing my own breakfast, and I have not found the coffee to be in the least inferior to that made in the most costly and complicated machines.

"The whole of this apparatus consists of a coffee-cup, which should hold about three quarters of a pint; and a strainer, made of tin, which is suspended in it by its brim. (See fig. 3.) This coffee-cup should be cylindrical, and, when employed in making one gill of good strong coffee, should be three inches in diameter within, and three inches and a half deep. The lower part of the strainer is one inch and a half in diameter, and one inch deep; and the upper part of it two inches and nine-tenths in diameter, and about one inch and a half in depth. The water which is poured on the ground coffee should be boiling hot; the cup and the strainer having both been previously heated, by dipping them into boiling water.

"When all the coffee has passed into the lower part of the cup the strainer may be taken away, and the cup may be covered with the cover of the strainer. I do not think it possible to contrive a more simple apparatus than this for making coffee, nor one in which coffee can be made in higher perfection.

"That represented by figure 4, which is of a size proper for making two cups of coffee, is equally simple; and as it may be made entirely of pottery, it would cost a mere trifle, perhaps not more than a shilling. The cup, which serves in two capacities, first as a reservoir in making the coffee, and then as a cup in drinking it, (and which, in a

family, may be used for other purposes,) is three inches and a half in diameter, internally, and four inches deep.

"As many persons may prefer coffee-pots made entirely of Staffordshire-ware, porcelain, or other pottery, to those made of the metals, not only on account of the low prices at which they may be afforded, but also on account of their superior neatness and cleanliness, I have added the figure 5, which, on a scale of half the full size, represents a coffee-pot made of pottery, of a size proper for making five or six cups of coffee at once, or three, four, five, six, seven, or eight cups, if two strainers are used, one after the other. When this coffee pot is used, it will be necessary to place it in boiling water to keep it hot, and it will be useful to cover the whole with a cylindrical vessel turned upside down; by which means both the strainer and the coffee-pot will be surrounded by hot steam, which will contribute very essentially to the goodness of the coffee. As soon as the coffee has passed into the coffee-pot, the strainer may be taken away; and the coffee-pot covered with the cover which is common to it, and to the strainer.

"I shall conclude by a few observations on the means that may be used for preserving ready made coffee, good for a considerable time, in bottles,

"The bottles having been made very clean, must be put into clean cold water, in a large kettle, and the water must be heated gradually, and made to boil, in order that the bottles may be heated boiling hot. The coffee, fresh prepared and still boiling hot, must be put into these heated bottles, which must be immediately well closed with good sound corks. The bottles must then be removed into a cool cellar, where they must be kept well covered up in dry sand, in order to preserve them from the light. By this means ready-made coffee may be preserved good for a long time; but great care must be taken not to let it be exposed to the light, otherwise it will soon be spoiled. When wanted for use, the coffee must be heated in the bottle and before the cork is drawn; otherwise a great deal of the aromatic flavour of the coffee will be lost in heating it. And in order that it may be heated in the bottle, without danger, the bottle must be put into cold water, and this water must be gradually heated till the coffee has acquired the degree of heat which is wanted. The cork may then be drawn, and the coffee poured out, and served up.

"As good coffee is very far from being disagreeable when taken cold, and as there is no doubt but it must be quite

quite as exhilarating when cold as when it is taken hot, why should it not be made to supply the place of those pernicious drams of spirituous liquors, which do so much harm?

"Half a pint of good cold coffee, properly sweetened, which would not cost more than half a pint of porter, would be a much more refreshing and exhilarating draught; and would no doubt be incomparably more nourishing.

"How much then must it be preferable to a dram of gin!

"The advantages and disadvantages to agriculture and commerce, which would arise from the introduction of a new beverage for supplying the place of malt liquors and ardent spirits distilled from grain, must be estimated and balanced by those whose knowledge of political œconomy fits them for determining these most intricate and important questions."

This ingenious Essay also presents descriptions (with engravings) of elegant coffee urns; but as these are destined for the opulent, we beg to refer for further particulars to the Count's Eighteenth Essay.

XVIII. *Some Remarks on the Use of Nitrat of Silver, for the Detection of minute Portions of Arsenic.* By Alex. Marcet, *M.D. F.R.S. one of the Physicians to Guy's Hospital**.

In the interesting account of the poisonous effects of arsenic, presented to the Society by Dr. Roget, and published in the second volume of the Medico-Chirurgical Transactions †, the author has recommended, for the detection of this poison, a test which I pointed out to him, and which, from a variety of experiments which we tried together, with a view to ascertain its comparative merits, we were induced to consider as the most effectual of all the tests hitherto used for that purpose. The method consists simply in adding in succession, to the fluid suspected to contain arsenic, minute quantities of solutions of ammonia and of nitrat of silver; by which means, if the smallest quantity of arsenic be present, a dense yellow precipitate will be produced.

* From the third volume of the Medico-Chirurgical Transactions, published by the Medical and Chirurgical Society of London.

† I take this opportunity of stating, at Dr. Roget's request, that the patient, whose case he there related, completely recovered her health, and has remained well ever since.

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All the particulars respecting this mode of detection having been fully stated by Dr. Roget, with such references to former writers on the subject as the case required, it would be quite superfluous to enter into any further detail on this head. My object in resuming the subject, the practical importance of which need not be pointed out, is to communicate to the Society the result of an inquiry which I have made on the nature of the yellow precipitate, the appearance of which is assumed as denoting the presence of arsenic, and to answer some objections which have been made against this test by Mr. Sylvester, of Derby, in a paper on metallic poisons, recently published in Nicholson's Journal*.

The yellow compound in question has the following properties:

If, after being well washed with distilled water, it be suffered to stand for some time in an open vessel, it gradually passes to a brown colour; but it does not, like nitrat of silver, become black on continuing this exposure.

It is readily soluble in dilute nitric acid. It also dissolves on adding an excess of ammonia at the moment of its formation; but after it has been separated and dried, it is no longer sensibly soluble in ammonia.

If a small quantity of this precipitate be exposed to the heat of a lamp on a slip of laminated platina, a white smoke arises from it, and metallic silver remains attached to the platina. The reduction of the silver, in the form of a globule, is still more distinct and striking, if a little carbonaceous matter be mixed with the precipitate, and the blowpipe applied.

When the yellow precipitate, inclosed in a tube, is exposed to the heat of a lamp, the white smoke condenses on the cold part of the tube, in minute octohedral crystals of arsenious acid.

It appears, therefore, that the precipitate in question is a combination of white arsenic (arsenious acid) and silver, or an arsenite of silver; and it is inferred that its formation, when ammonia and nitrat of silver are added to a mixture containing arsenious acid, is owing to a double elective decomposition of the arsenite of ammonia by the nitrat of silver, in consequence of which arsenite of silver is formed, and separates as an insoluble precipitate from the nitrat of ammonia which remains in the solution. The addition of ammonia is necessary, because arsenic acid alone cannot

* Nicholson's Journal for December 1812, vol. xxxiii. p. 306.

decompose

decompose nitrat of silver; but in Fowler's solution, in which the arsenic is already combined with an alkali, the decomposition takes place at once, without any addition of ammonia. The fixed alkalies, therefore, can answer a similar purpose; but ammonia has this advantage, that it does not, when added singly, decompose nitrat of silver,—a circumstance which, in using the fixed alkalies, might occasion some confusion*.

With regard to Mr. Sylvester's objection, I shall, previously to my offering any remarks upon it, state it in his own words: "If ever muriatic acid be present," says this gentleman, "the test is then wholly useless, as a muriat of silver will be immediately formed, and the yellow compound, said to be so unequivocal in its indication of arsenic, of course be prevented from appearing."

This danger of ambiguity, however, though applying in some degree to the process in question, and well deserving to be noticed, will be found to have been greatly overrated; and there are such easy and obvious means by which this ambiguity can be entirely removed, that it can make no solid objection to the utility of the test.

There cannot be the least doubt, as Mr. S. observes, but that whenever nitrat of silver is added to a solution containing muriatic acid, a precipitate of muriat of silver must be the consequence. But if the nitrat of silver be added in excess, the arsenite of silver is also thrown down by the intervention of ammonia, and a mixed precipitate of luna cornea and arsenite of silver is obtained, which partakes more or less of the yellow colour of the latter, according to the proportion of the two salts.

If to this dubious precipitate a few drops of dilute nitric acid be added, the arsenite of silver is instantly dissolved, and the muriat of silver, which is insoluble, immediately resumes its peculiar density and whiteness. If a little ammonia be now added to the clear fluid, the yellow precipitate appears in the most distinct manner, and becomes even more characteristic from a comparison with the white pre-

* It is necessary, as Dr. Roget has observed in the paper already quoted, that the quantity of ammonia should not be too large; for in that case the precipitate is redissolved. But, even then, it may be made to reappear, by the addition of nitric acid in sufficient quantity to saturate the alkali. In this case, however, the precipitate is not permanent, owing, I find, to its being soluble in the nitrat of ammonia which is formed in the process. Carbonat of ammonia has also the property of producing and redissolving the precipitate.

The fixed alkalies in excess have not the power of redissolving the precipitate.

cipitate,

cipitate, the appearance of which differs from this in every respect.

By this method, I believe that every objection to the test will be removed; and in order to anticipate all ambiguity, and to avoid any complication or practical difficulty in its application, I would propose to modify the process in the following manner:

To the suspected fluid, previously filtered, add, first, a little dilute nitric acid, and, afterwards, nitrat of silver, till it shall cease to produce any precipitate. The muriatic acid being thus removed, whilst the arsenious acid (if any, and in whatever state,) remains in the fluid, the addition of ammonia will instantly produce the yellow precipitate in its characteristic form. It is hardly necessary to add, that the quantity of ammonia must be sufficient to saturate any excess of nitric acid which the solution may contain.

XIX. *On a Periscopic Camera Obscura and Microscope. By* William Hyde Wollaston, *M.D. Sec. R.S.**

Although the views which I originally had of the advantage to be derived from the periscopic construction of spectacles† naturally suggested to me a corresponding improvement in the *camera obscura*, by substituting a meniscus for the double convex lens, I have hitherto deferred making it known to others, except as a subject of occasional conversation.

Since in vision with spectacles, as in common vision, the pencil of rays received by the eye in each direction is small, the superiority of that form of glass, which disposes all parts of it most nearly at right angles with the visual ray, admits of distinct demonstration: but with respect to the camera obscura, where the portion of lens requisite for sufficient illumination is of considerable magnitude, although it is evident that some improvement may be made in the distinctness of oblique images on the same principles; yet as the focus of oblique rays is far from being a definite point, the degree in which it may be improved is not a fit subject of mathematical investigation.

I have therefore had recourse to experiments, in order to determine by what construction the field of distinct representation may be most extended; and I trust the result will be acceptable to this Society. I shall take the same

* From the Philosophical Transactions for 1812, part ii.

† Phil. Mag. vol. xvii. Nicholson's Journal, vii. 143.

opportunity

opportunity to describe an improvement in the construction of the simple microscope, which may also be termed periscopic, as the object of it is to gain an extension of the field of view, upon the same principles as in the preceding instances, namely, by occasioning all pencils to pass as nearly as may be at right angles to the surfaces of the lens. The mode, however, in which this is effected is apparently somewhat different in the practical execution.

In the common *camera obscura*, where the images of distant objects are formed on a plane surface to which the lens is parallel, if the surfaces of the lens be both convex, and equally curved (as in fig. 1. Plate IV); and if the distance of the lens be such, that the images formed in the direction of its axis CF be most distinct, then the images of lateral objects are indistinct in a greater or less degree, accordingly as they are more or less remote from the axis. The causes of this indistinctness may be considered as twofold; for, in the first place, all parts of the plane, excepting the central point, are at a greater distance from the centre of the lens than its principal focus; and secondly, the point *f*, to which any pencil of parallel rays passing obliquely through the lens are made to converge, is less distant than the principal focus. On this account, it is in general best to place the lens at a distance somewhat less than that which would give most distinctness to the central images, because in that case a certain moderate extension is given to the field of view, from an adjustment better adapted to lateral objects, without materially impairing the brightness of those in the centre. The want of distinctness, however, is even then only diminished in degree, but is not remedied.

The construction by which I propose to obviate this defect is represented in the second figure, in which are seen the essential parts of a periscopic camera in their due proportion to each other. The lens is a meniscus, with the curvatures of its surfaces about in the proportion of two to one, so placed that its concavity is presented to the objects, and its convexity toward the plane on which the images are formed. The aperture of the lens is four inches, its focus about twenty-two. There is also a circular opening, two inches in diameter, placed at about one-eighth of the focal length of the lens from its concave side, as the means of determining the quantity and direction of rays that are to be transmitted.

The advantage of this construction over the common camera obscura is such, that no one who makes the comparison can doubt of its superiority; but the causes of this

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may require some explanation. It has been already observed, that by the common lens, any oblique pencil of rays is brought to a focus at a distance less than that of the principal focus. But in the construction above described, the focal distance of oblique pencils is not merely as great, but is greater than that of a direct pencil. For since the effect of the first surface is to occasion divergence of parallel rays, and thereby to elongate the focus ultimately produced by the second surface, and since the degree of that divergence is increased by obliquity of incidence, the focal length resulting from the combined action of both surfaces will be greater than in the centre, if the incidence on the second surface be not so oblique as to increase the convergence. On this account, the opening E is placed so much nearer to the lens than the centre of its second surface, that oblique rays E*f*, after being refracted at the first surface, are transmitted through the lens nearly in the direction of its shorter radius; and hence are made to converge to a point so distant that the image (at *f*) falls very nearly in the same plane with that of an object centrally placed.

In the use of spectacles by long-sighted persons, the course of the rays in the opposite direction is so precisely similar, that the same figure might serve to illustrate the advantages of the periscopic construction. For the purpose of seeing the extended page of a book (as at AB) with least fatigue to the eye, that form of lens will be most beneficial, which renders the rays received from each part of its surface parallel; and this is effected by the exact counterpart to the preceding arrangement; for in this case the opening E represents the place of the eye receiving parallel rays from the lens in each direction, instead of transmitting them from a distance towards it.

There is, however, this difference between the two cases, that in the camera obscura a much larger portion of the lens is required to conspire in giving a distinct image of any one object; so that the conformation best adapted for lateral objects would not be consistent with distinctness at the centre; and hence arises a limit to the application of the principle. On the common construction, the whole lens is so formed as to give brilliancy and distinctness at the centre alone, without regard to lateral objects. In adopting such a deviation from the customary form, as I propose, in favour of a more extended view, some diminution of the aperture is required in order to preserve the desired distinctness at the centre. In my endeavours to ascertain the most eligible form of meniscus for this purpose,

pose, I have assumed sixty degrees to be the field of view required. But when so large a field is not wanted, then a lens that is less curved will be preferable; and the proportion of the radii must be varied according to the angular extent intended to be included.

For the purpose of estimating by what combination of radii any required focal length may be given to a meniscus, I have contrived a diagram by which very much labour of computation may be saved, as a very near result may be obtained by mere inspection. This contrivance is founded on the well known formula for the focal length of any lens $F = \frac{mrR}{R \pm r}$: m being a certain multiple obtained by dividing the sine of refraction by the difference of the sines of incidence and refraction. Hence, in applying this formula to the meniscus, $F : R :: mr : R - r$. In fig. 3, lines expressive of these quantities are so arranged, that by assuming any point F corresponding to the focal length desired, and drawing a line FR through a point R indicating any supposed length of the greater radius, the corresponding length of the other radius will be found where the line drawn intersects the middle line in the diagram.

In laying down these lines, the length and position of AF and AR were assumed at pleasure; and they were divided into any number of equal parts. But the position and length of the middle line Ax was adapted with care to the refractive power of plate glass in the following manner. Since $m = \frac{1}{1{\cdot}505 - 1} = 1{\cdot}98$, a line BC was drawn from the point 10 in the line AR, parallel to AF, and equal to 19·8 divisions of the primary lines; so that if r be $= 10$, then the line $BC = mr$. The distance AC being then divided into ten equal parts, with their subdivisions, afforded the means of continuing the same scale to any desired length. Since the first line BC was laid down parallel to AF, and equal to mr, any other lines drawn through corresponding numbers 7 and 7, 8 and 8, &c. will be also parallel, and, by preserving due proportion, will correctly represent mr. Hence, in all positions of the line FR, the same similarity of triangles obtains, and the same proportion of $F : R :: mr : R - r$; and consequently the focal length, corresponding to any assumed radii, is truly ascertained.

For the purpose of duly proportioning the curvatures of flint-glass, a second line Ay might be laid down in a mode

mode similar to the preceding, by adapting the multiple $m = \frac{1}{1{\cdot}58 - 1} = \frac{19}{11}$ to the different density of this glass.

With respect to the construction of a microscope on periscopic principles, I believe the contrivance to be equally new with the former, and equally advantageous. The great desideratum in employing high magnifiers is sufficiency of light; and it is accordingly expedient to make the aperture of the little lens as large as is consistent with distinct vision. But if the object to be viewed is of such magnitude as to appear under an angle of several degrees on each side of the centre, the requisite distinctness cannot be given to the whole surface by a common lens, in consequence of the confusion occasioned by oblique incidence of the lateral rays, excepting by means of a very small aperture, and proportionable diminution of light.

In order to remedy this inconvenience, I conceived that the perforated metal, which limits the aperture of the lens, might be placed with advantage in its centre; and accordingly I procured two plano-convex lenses ground to the same radius, and applying their plane surfaces on opposite sides of the same aperture in a thin piece of metal (as is represented by a section, fig. 4), I produced the desired effect; having virtually a double convex lens so contrived, that the passage of oblique pencils was at right angles with its surfaces, as well as the central pencil. With a lens so constructed, the perforation that appeared to give the most perfect distinctness was about one-fifth part of the focal length in diameter; and when such an aperture is well centred, the visible field is at least as much as twenty degrees in diameter. It is true that a portion of light is lost by doubling the number of surfaces; but this is more than compensated by the greater aperture, which, under these circumstances, is compatible with distinct vision.

Beside the foregoing instances of the adaptation of periscopic principles, I should not omit to notice their application to the camera lucida; as there is one variety in its form, that was not noticed in the description which I originally gave of that instrument *.

In drawing, by means of the camera lucida, distant objects are seen by rays twice reflected (*d*, fig. 5), at the same time and in the same direction that rays (*e*) are received

* Phil. Mag. xxvii. p. 343. Nicholson's Journal, xvii. p. 1.

from

from the paper and pencil by the naked eye. The two reflections are effected in the interior of a four-sided glass prism, at two posterior surfaces inclined to each other at an angle of 135 degrees. In the construction formerly described, the two other surfaces of the prism are both plane, through which the rays are simply transmitted at their entrance and exit. But since an eye that is adjusted for seeing the paper and pencil, which are at a short distance, cannot see more distant objects distinctly without the use of a concave glass, it may be assisted in that respect by a due degree of concavity given to either, or to both the transmitting surfaces of the prism. It is, however, to the upper surface alone that this concavity is given; for, since the eye is then situated on the side toward the centre of curvature, it receives all the benefit that is proposed from the periscopic principles.

XX. *M.* Figuier's *new Process for depriving Vinegar and other Vegetable Liquids of their Colour* *.

The agent employed is animal charcoal, and the process is easy, œconomical, and may be applied with equal facility in the large as in the small way. To take away the colour of vinegar, a litre of the red kind (red wine vinegar) cold, is mixed with 45 grammes of bone charcoal, in a glass vessel: this mixture is shaken from time to time, and in two or three days the colour disappears so completely that when filtered through paper it passes perfectly limpid, without having lost any of its taste, smell, or acidity. When the process is to be performed in the large way, the charcoal is thrown into a cask of vinegar, which must be stirred from time to time to renew the points of contact. In the large way not above half the proportion of charcoal is required as for small quantities: the colour does not vanish so instantaneously, but the result is certain, nor does the length of time the vinegar is left in contact with the charcoal at all injure it.

Vinegar thus rendered limpid may be rendered aromatic by infusing plants in it before discoloration, or by mixing with it afterwards a small quantity of alcohol charged with the aromatic principle. It is then preferable to any other vinegar for the table, the toilet, pharmacy, and pickling green fruits.

The highest coloured red wines treated in the same

* Abridged from *Ann. de Chim.*

 manner

manner become perfectly limpid, retaining uninjured their smell and taste.

In a similar manner the acid residuum from the preparation of sulphuric ether may be perfectly deprived of colour. The residuum, mixed with an equal weight of water, is filtered through paper (placed in a glass funnel and supported by a small piece of cloth placed in the neck) to separate the carbonaceous and oily matter formed by the action of the acid upon the alcohol. If 50 grammes of bone black be mixed with a litre of the filtered acid, in a matrass, agitated from time to time, at the end of two or three days, on filtering the mixture, the acid will pass through perfectly colourless. By this means almost the whole of the acid employed in the preparation of ether may be recovered, and the acid (when evaporated to drive off the water) may be employed for any use to which sulphuric acid is applied.

Tincture of turnsole, mixed with a small quantity of animal charcoal, speedily loses its colour.

The charcoal is prepared as follows: Fill a crucible with the most compact parts of ox and sheep bones; lute the cover carefully, leaving only a small opening at the top; place the crucible on a forge fire, and heat it gradually till red: when the flame from the oily and gelatinous parts of the bones has ceased, diminish the opening and suddenly raise the fire:—carburetted hydrogen gas and oxycarburet will then be evolved. When cold reduce the charcoal, on porphyry, to a fine powder.

Ivory black possesses the same property as bone black. In a word, all charcoals prepared from animal substances by calcination in close vessels, answer for this purpose.

XXI. *Notice respecting some Experiments on Alcohol; read before the Edinburgh Institute 2d February* 1813. *By Mr.* Hutton.

I have been prevailed upon to communicate a notice of some experiments and observations I have made on the production of a great degree of cold. It is scarcely necessary to observe, that my doing so at this time is not a matter of choice: these experiments and observations were mentioned to my friends; and they were made without any injunction as to secrecy, as I did not anticipate that such communications would either be received with so much avidity, or repeated with so much eagerness. The consequence

quence has been, that accounts of these experiments have now got into very general circulation, and many very contrary and erroneous ideas have been entertained, not only as to their extent, but even as to their nature: and it has been imagined that a communication like the present is the only way to obviate these misconceptions,—misconceptions which I owe as much to you as to myself to remove.

The importance of a method of producing a great degree of cold becomes apparent, when it is considered that it is at present a very common opinion among chemists,—an opinion founded on a very general analogy,—that all gases may be reduced to the state of liquids by the abstraction of caloric; and that, by a further abstraction of caloric, all liquids in their turn may be reduced to the solid state. If this be true, and were we in possession of a method of sufficiently abstracting caloric, all bodies whatever might be reduced to the solid state. We should thus become acquainted with a great number of substances that we have hitherto had no opportunity of examining; many powerful agents would likely be obtained; many new and interesting compounds formed; and much light could not fail to be thrown on the constitution of known substances.

Directing my attention to this subject, in the summer of 1810, a method occurred to me, by which I imagined a greater degree of cold might be produced than had hitherto been obtained. Although the power of this method appeared in theory almost indefinite, yet it was easy to foresee that in practice many circumstances might at first concur to set limits to its application: from the nature of these circumstances, however, it was to be expected that some of them might be considerably modified, and many of them might in time be altogether removed, and thus the practice made in some degree to approximate to the theory.

At the time this method occurred to me, the pressure of my professional avocations did not allow me to prosecute it; but, as I anticipated some leisure in the following autumn, I immediately began to provide, at any leisure moments I had, such apparatus as I considered absolutely necessary, or was most likely to be useful. The little dependence, however, which is to be placed on general reasoning on such subjects, and the apprehension that the method might have been previously tried and found insufficient by others, prevented me from providing any very extensive apparatus.

My first experiment was tried in the following autumn.

 The

The thermometer was filled and sealed by myself. The tube was previously tried by the common method, and found, as nearly as such tubes are commonly to be met with, of equal calibre throughout. The spirit with which it was filled was prepared by Richter's process, and afterwards re-distilled by itself.—Its specific gravity at 62° was 798.—The points 60° and 100° were determined by a mercurial thermometer which had been made with the usual precautions; the interval was divided into four spaces, each of which, of course, corresponded to 10°; the part of the stem below 60° measured nearly 18 of these spaces. A mark was made at every space, till, on arriving at the end of the 17th, the graduation could not be carried further. This point, of course, corresponded to $+60° - 170° = -110$ deg. of Fahrenheit's scale.

This thermometer was exposed to the cold produced by the method alluded to, and after some time was examined, when the alcohol was found to have passed all the marks, and was obviously sunk within the ball of the thermometer. A slight degree of discoloration was observable. The thermometer was replaced, and examined about five minutes afterwards, when the ball of the thermometer was found broken, and crystals adhered to the fragments.

I next took a glass tube about 3-10ths of an inch in diameter, and sealed at one end; into this I poured alcohol till it stood in the tube 4-10ths of an inch deep, and then exposed it to the cold, produced as before: after some time it was so completely solid, that on inverting the tube it did not drop, and only a very minute stream was perceived to glide slowly down the inside of the tube: when this stream had reached nearly the middle of the tube, the whole suddenly fell out, and, pitching in a glass, was broken into several pieces, which quickly melted.

This experiment was several times repeated; but by allowing the alcohol to remain a little longer exposed to the cold it became so completely solid, that on inverting the tube, not the least portion of fluid could be perceived to separate from the mass.

In order to be as certain as possible of the strength of the alcohol I employed, I again took its specific gravity, and the result corresponded with what I before obtained.

These experiments, therefore, left me no room to doubt that I had frozen alcohol, which, at the temperature of 62°, is of the specific gravity 798.

Being appointed to deliver the Course of Lectures on Chemistry for the Session 1810-11, I had no leisure at that

that time to pursue these experiments. They were resumed, however, in the autumn of 1811. The second experiment was repeated and varied, and solid masses of alcohol of some magnitude obtained. Some of these I soldered together, using as a hot bolt a rod of frozen mercury, and sometimes a straw cooled down to a very low temperature.

It now appeared to me to be an object of some importance to ascertain the form of the crystals which this substance assumes. This I found attended with some difficulties which I did not anticipate, and attempts to overcome them have led to the discovery of some facts which I did not at all expect.

The common masses exhibited crystals of different forms; two kinds appeared to predominate, and each was tolerably distinct in its kind; but it was not very easy to perceive by what increments or decrements the one could be supposed to pass into the other: a rather casual circumstance, however, explained the source of this variety.—Attempting to freeze alcohol by a modification of the general process, which I conjectured would yield more regular crystals than the common method, I observed that, before crystallizing, the alcohol separated into three very distinct strata; the uppermost was of a pale yellowish green, while the second was of a very pale-yellow colour:—both these strata were very thin, the last-mentioned was rather the thickest; the lowermost stratum was nearly transparent and colourless, and very greatly exceeded the other two in quantity. After allowing a part of the lower stratum, which I conceived to be the pure alcohol, to freeze, I attempted to pour out the remainder, but was prevented by the upper strata, which proved to be solidified. The lowermost of these two strata bore some marks of crystallization—the upper had none, and proved so firm as to resist a straw with which I attempted to perforate it to open a passage for the sublatent liquid. On removing part of these superior strata, and decanting the remaining fluid, the crystals of the lower stratum appeared very distinctly to be rectangular prisms of equal planes, a few of them on one side of the glass surmounted by quadrangular pyramids, but most of them by dihedral summits. This experiment I repeated several times, and the results coincided.

In order to ascertain whether these phænomena arose from a decomposition of the alcohol, or from the separation of foreign substances previously held by it in solution, the products of several of these experiments were mingled together in a stoppered matrass; the whole was then raised

 to

to the temperature of about 120 deg. by a water bath of that temperature. The substances forming the different strata united together, and formed a colourless liquor, which had the specific gravity and all the other properties of the alcohol from which it was obtained. This experiment was repeated several times, and the results were uniform, affording sufficient evidence that the alcohol had not been decomposed by this process, but that the superior strata consisted of foreign substances which it had held in solution. The variety in the form of the crystals obtained by former experiments, was therefore most likely occasioned by the presence of these foreign substances,—a phænomenon not uncommon in chemistry.

The result of these experiments led me now to perceive, that the assumption that alcohol, prepared by Richter's process, is perfectly pure, or at most contains only a very minute portion of water, is entirely gratuitous. The diluted alcohol of commerce, from which the more concentrated is obtained, is well known to contain different volatile impurities; and since Richter's process makes no provision for the separation of these, we ought rather to expect still to meet with some portion of them in alcohol prepared in this manner.

I next proceeded to examine the properties of the different substances into which I had separated Richter's alcohol: but the time I had now left for this purpose was too short for making much progress in this inquiry; a few only of their habitudes with water, and with one another, were all that I had time to examine; even these I could examine only imperfectly.

The lowermost stratum, or nearly colourless fluid, which I have called alcohol, had no flavour, and produced on the organ of smell only a sharp pungent sensation. It has the remarkable property of smoking when exposed to the air, and when diluted with water it differs considerably in taste from common diluted spirit of wine.

The pale yellow substance, or second stratum, has a pungent taste, leaving an impression of sweetness. It has a very strong but agreeable smell. When mixed with the alcohol, and diluted with water, it has very much the flavour or the better kinds of Highland whisky. It readily dissolves in water, and communicates to that fluid its peculiar flavour.

The pale yellowish green substance which composes the uppermost stratum, has a strong and very offensive smell, and a very sharp nauseous taste. It dissolves in alcohol, to

to which it communicates its peculiar flavour; its disagreeable smell is considerably heightened by this combination. It dissolves in water, though less readily than the substance last treated of. The compound, when much diluted and heated, has very much the flavour of the *low-wine* of our Lowland distillers at the time when it issues from the still.

The two last-mentioned substances, or those of which the two upper strata are composed, when mixed together and greatly diluted with water, have very nearly the flavour of alcohol. They have rather more volatility than water; for, when half of a solution of them has been distilled over, the distilled part has a much stronger smell than that which remains in the retort.

It may be proper to mention, that from the circumstance of my sense of smell having been for some time extremely obtuse, I have been under the necessity of trusting to others for the facts regarding the flavour of these new substances and mixtures: from the uniformity of the reports, however, which I have received from different persons, I have no doubt that these facts are correct.

Besides that from which I filled the thermometer in the first experiment, I have operated on alcohol of the specific gravities 802, 797, and 784; the specific gravity of the last was taken when its temperature was 66 deg. and it is probably the most concentrated that has ever been obtained. But, with alcohol of all these different strengths, the general results were similar. In alcohol obtained from different sources, the proportions of the impurities were different, both with regard to the pure alcohol and to one another; but I have met with none that did not contain both.

From these experiments I think it is ascertained,

1st, That the strongest alcohol which we are able to obtain may be frozen by the method alluded to.

2d, That this alcohol contains at least two foreign substances, which are highly volatile, and, so far as is known, can only be separated by freezing.

3d, That it is to these substances that alcohol owes its peculiar flavour; and that, according as the one or other predominates, the flavour of the alcohol is agreeable or otherwise.

Last autumn I resumed this subject, and my attention was chiefly directed to the habitudes of these impurities with the chemical re-agents. This I found attended with considerable difficulties, none of the least of which was to procure a sufficient quantity of these impurities in a sepa-

 rate

rate state. The series of experiments I proposed to myself on this subject have not yet been completed; but I may remark, that the result of some of those I have made promises to afford practical hints of considerable importance to those brewers whose products are intended to afford spirituous liquors.

From this notice it will be observed, that I have scarcely yet entered on the wide field of inquiry, for cultivation of which, the method alluded to appears to offer so powerful an instrument. Alcohol only has been subject to experiment; it was the only liquid which had resisted all attempts to reduce it to the solid state by the abstraction of caloric. If these experiments be correct, we may now pronounce it a general law to which there is no exception, that all liquids with which we are acquainted may be reduced to the solid state by a suitable abstraction of caloric. Whether all gases may be susceptible of reduction to the solid state, by the abstraction of caloric, remains to be ascertained; although, as I have mentioned, analogy renders it in the highest degree probable.

The examination of the singular substances which alcohol prepared by Richter's process contains, has drawn me aside from the course of experiments I prescribed to myself, and taken up that time which I intended to have devoted to the examination of the effects of cold on the gaseous bodies. Whether I shall proceed to these bodies, or resume the examination of the habitudes of the alcoholic impurities with the re-agents, will much depend on the leisure which I can obtain; but, to whichever of them I may direct my attention, I shall not fail to give the earliest information of the result to the Institute.

XXII. *A Comparative Scale of the Thermometers of Celsius, or the Centigrade,—Reaumur,—Fahrenheit, and Walker.*

To Mr. Tilloch.

Sir,—At the suggestion and request of a medical gentleman, eminent in his profession, and highly esteemed for his knowledge of science in general, I drew up the table annexed.

Should you consider a copy from it not undeserving of a place in the next number of your Magazine, I request you will have the goodness to insert one; presuming myself, that it might form a useful appendage to a paper of mine on

on the same subject, which you did me the favour to insert in the Philosophical Magazine for June 1810.

I am, sir, your obedient servant,

Oxford, Feb. 12, 1813. RD. WALKER.

COLUMN I.

HEAT.

	C.	R.	F.	W.	
W.	100.	80.	212.	150.	B.
			200.	140.	
	90.	70.	190.	130.	
	80.		180.	120.	
		60.	170.	110.	
	70.		160.	100.	
		50.	150.	90.	
	60.		140.	80.	
		40.	130.	70.	
	50.		120.	60.	
			110.	50.	
	40.	30.	100.	40.	
	30.		90.	30.	
		20.	80.	20.	
	20.		70.	10.	
M.	16¼.	13½.	62.	0	T.

COLUMN II.

COLD.

	C.	R.	F.	W.	
M.	16¼.	13½.	62.	0.	T.
	10.	10	50.	10.	
			40.	20.	
W.	0.	0.	32.	30.	F.
			20.	40.	
	10.	10.	10.	50.	
	20.		0.	62.	
			10.	70.	
	30.	20.	20.	80.	
			30.	90.	
	40.	30.	40.	100.	
			50.	110.	
	50.	40.	60.	120.	
	60.		70.	130.	
		50.	80.	140.	
G.	68.	54½.	91.	153.	C.

N.B. One degree on the thermometer of Reaumur is equal to $1\frac{1}{5}$ on that of Celsius, or the Centigrade; and to $2\frac{1}{4}$ on those of Fahrenheit, and Walker: hence it follows, that *every fourth decimal*, on the thermometer of Reaumur, is coincident with *every fifth decimal*, on that of Celsius, or the Centigrade; and with *every ninth decimal*, on that of Walker; the decimals of F. being *each*, *one point*, lower than those of W.—Attention to these circumstances will ever serve to regulate this table.—The *second column* is a continuation of the first.

The scale commences at the *boiling point* of water, and terminates at the greatest degree of cold, art has hitherto produced. M. T. is mean temperature; viz. that temperature at which the human body, in a state of health, and at rest, is *unconsious of either heat or cold*.

For a *comparative account* of these four thermometers, see Phil. Mag. for June 1810, p. 416.

XXIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Jan. 28. THE Right Hon. President in the chair. A letter from Dr. Brewster to Sir Humphry Davy was read, announcing some important discoveries in the double refraction and dispersing powers of several substances, as agate in a thin plate about 1-15th of an inch thick; chromate of lead, sulphur, fluor spar, Iceland spar, &c. The first two of these minerals, it appears, exceed the diamond in double refraction and also in dispersion. Dr. Brewster refers to the experiments of Malus on colours, and expresses a hope that his own discoveries in this difficult branch of science may lead to some correct theory of light: but he is still pursuing his experiments, which are not yet in a state for generalization.

Feb. 4. Sir Charles Blagden communicated a short paper on Near-sightedness, confirming the observations of Mr. Ware, that the early use of concave glasses contributes to injure the sight. Sir C. stated his own case, being like Mr. W. near sighted: this he discovered when only nine years old, and used watch-glasses to aid his sight: at length requiring proper glasses, at the age of 30 he used No. 2; a few years after he found it necessary to adopt No. 5, in order to obtain a clear view. Since that period his sight has remained stationary; but he is inclined to think that had he abstained from the use of glasses, it would have become sufficiently long and clear by exercise,—such is the facility

facility with which the eye adapts itself to perceive near or distant objects.

Feb. 11. A letter to the President from Mr. Hamilton of Nevis was read. It contained a long account of the eruption of the Souffriere in the island of St. Vincent, in May 1812. This volcano had not experienced an eruption since 1718; the recent one was preceded by near 200 shocks of earthquakes during the twelve months before May. The most particular phænomenon noticed by the writer was the sound of the eruptions, which so much resembled the alternate firing of cannon and small arms, that the captain of a ship of war convoying a fleet of merchantmen, conceiving that a privateer had attacked some of the rear vessels, made signal to the fleet to close, and steered towards the place whence the sound came. It was also remarked, that the noise was much greater at the distance of many leagues than it was in the island; a circumstance for which Mr. Hamilton could not account. By this eruption two rivers were dried up; immense volumes of thick smoke were emitted before any flame appeared at the mouth of the crater; the flame was accompanied by successive shocks of the earth, thundering noise, and the discharge of large pieces of pumice during eight hours, without intermission. Several houses were thrown down in Kingston by the tremor, and many negroes were wounded by the pumice which struck them in their plantations. The Souffriere is in a part of a great chain of mountains which pass through Nevis and several other of the West India islands. Its crater is a mile in diameter, and about 900 feet deep.

Feb. 18. In consequence of the indisposition of the President, Mr. Lysons was in the chair. A paper by Sir Everard Home was read, describing the head of the Narval. Mr. Scoresby junior having informed him that the female narval has no tusk, and that it is a vulgar error to believe it such a deadly enemy to the whale, Sir E. examined the head of one in the Hunterian museum, and compared it with the head of a female, which he procured.

He observed that the head of the male has a socket for two tusks, but has only one, while that of the female has a place for one, but has none. Hence he thinks the question beyond all doubt, and that Mr. Scoresby's information was correct, that the female narval never has any tusks.

Dr. Wollaston communicated the result of his experiments in drawing wire. Having required some fine wire for telescopes, and remembering that Muschenbroek mentioned

tioned wire 500 feet of which weighed only a single grain, he determined to try the experiment, although no method of making such fine wire has ever yet been published. With this view he took a rod of silver, drilled a hole through it only 1-10th its diameter, filled this hole with gold, and succeeded in drawing it into wire till it did not exceed the 3 or 4000dth part of an inch, and could have thus drawn it to the greatest fineness perceptible by the senses. Drilling the silver he found very troublesome, and determined to try to draw platina wire, as this metal would bear the silver to be cast round it. In this he succeeded with greater ease, drew the platina to any fineness, and plunged the silver in heated nitric acid, which dissolved it, and left the gold or platina wire perfect. The process Dr. W. thinks may be rendered useful to manufactures.

PHILOSOPHICAL SOCIETY OF LONDON.

Mr. Wright's Lectures on the Passions: forming the second Course of his Elucidation of the Oratorical Character. [Continued from p. 59.]

Pursuing his sixth Lecture, and descanting upon the doctrine of sound, and the affinity between it and motion, with the power it possesses of imitating the latter; Mr. Wright made among others the following observations:— "Sound," said he, "is not only imitative of motion, but also, though remotely, is often characteristic of the nature and importance of the body moving. And further, such is the analogy between sound and motion, that we seem intuitively to make use of similar braced or unbraced motions of the hand, in repeating rhetorically the words 'soft and loud,' or 'slow and quick,' or 'low and high:' *i. e.* in pronouncing either of the words 'soft, slow, or low,' similar motions of the hand are adopted; and also in pronouncing the words 'loud, quick, or high:' in the former the action of the muscles is relaxed, in the latter it is braced. The low expression of voice and extended gesture accompanying the description of any object, vast, unwieldy, accord with nature, and the practice of the best composers of music. If of diminutive objects, in proportion, the reverse. Solemnity of idea is well displayed by a slow and stately march of syllables: liveliness, by short syllables and sprightliness of measure. In the pronunciation of the first species of language, grave or flat tones are inseparable from a just delivery of it; in the latter, sharp or acute."

Some learned and judicious observations succeeded, on the mystical union of soul and body; and on the correspondence between the powers of the former with the appearance

pearance of the latter. The consideration of this almost inscrutable topic induced the lecturer (whose business only was to apply to his present object so much of it as is most easily recognizable) to notice "our habitual propensity to prejudge the appearance of persons, in proportion to our knowledge of the energies of their minds." From this natural (and generally expected) coincidence the lecturer concluded with intimating the necessity, "where Nature has been less bountiful of external accomplishment than might be desired, of the culture of mind, and of recourse to all the aids and advantages derivable from art and science, with tenfold ardour and perseverance."

Having thus discussed the advantages derivable from an intimate acquaintance with the powers of articulate sound and of gesture, as adaptable to the various modifications of passion, Mr. W. advanced more fully into his subject, and proceeded to elucidate the different characters of Expression; at the commencement of which elucidation he delivered a needful (and to us in some measure a novel) definition of the distinction between *reading* and oratory. "In relation to Expression and Feeling," said he, "an orator may occasionally appear in four distinct situations. First, He may be the Actor or Imitator; secondly, the Describer; thirdly, the Narrator; and lastly, whether imitated, described, or narrated by an author, he may be the Reader of a circumstance: so that one and the same individual passion may assume six different forms or modes of expression." This principle the lecturer ably enforced by his manner of reciting the inspired Ode of Collins on the Passions; in commenting upon which he displayed considerable critical acumen, while he elucidated the reference it bore to some of his own leading positions. He extolled in strong language the vivid and unequivocal delineations of character exhibited by the poet. "In every picture of passion (said Mr. Wright) therein presented, the prominent and peculiar features of each are so faithfully preserved, that the action almost appears present to the mind. The sloth-like caution of Fear, the pride and impetuosity of Anger, and the long incoherent pause of Despair, are exemplified by the hand of a master: and the sweetly-flowing numbers of Hope, moving like the 'golden hinges' of Milton, are interrupted with almost unequalled force of contrast by the loud ravings and boisterous exultation of Revenge.

"The exordium of this ode," continued the lecturer, "invests the speaker with the character of Describer. Care therefore should be taken to avoid an actual *imitation* of the passions

passions therein delineated: it would be departing from the intention of the Ode. It is required only that the student should bear in mind, while he echoes the sense, that the expression of his voice and gesture is for the *information only* of the auditor: consequently his whole deportment should assume the air of communication. If the repeater be only a reader, then his relation to the original action will be more remote: He will not be supposed to have seen the circumstance; consequently his expression of the passion should be proportionably less animated. Yet, although 'the page prescribed' before him must, in some measure, restrain the manner of the reader, it becomes him to infuse a resemblance of character into his delivery; otherwise the whole will be blended into one uniform, monotonous expression of tranquillity.

"In reading, the signs of passion are not so forcibly expressive as in repeating from memory; and for reasons appearing perfectly analogous to nature. A reader cannot be supposed to know what turn of thought an author may have taken, until he has actually rounded his period. He is only in possession of the growth of idea, or, in other language, of the meaning of such portions of words, forming parts of a period, as through the medium of the auditory organ may be clearly conveyed to the mind. Now, when we consider the nature of some of these portions, and the aptitude of the mind to receive impression of completion however false in point of logical accuracy, we shall be more fully convinced of the propriety of what is now advanced; viz. that it is highly requisite for the reader to restrain his feelings. For as a written theme," continued Mr. W. "as opposed to oratory, is produced by more deliberate acts of the mind; so should reading, as opposed to the higher branches of elocution in the various modes of utterance, bear no more than suitable proportions of energy and pathos."

From these and subsequent observations the lecturer demonstrated, that, as in narrating the probable motives of a transaction, description of the act would be irrelevant; so, in describing the apparent feeling of the actor in such transaction, it would not be decent for the orator to assume his attitude or supposed gesture.

"The peculiar properties," resumed the lecturer in his seventh discourse, "of Narration, Description, and Imitation, and their relation to each other, being recognized, will enable the student to ascertain the distinct characters of Expression." Dividing, then, this Expression into two grand

grand classes, Dramatic and Oratorical, Mr. W. proceeded to maintain the propriety of this division, and to interest his hearers by a wide range of citation from our critical, metaphysical, and dramatic writers. Nor did the annals of senatorial eloquence pass unheeded, enriched as the page has been by the stupendous talents of a Chatham,—a statesman and orator whose luminous and capacious intellect, ardently employed in the cause of truth and humanity, has left so decided a claim on the admiration and gratitude of his countrymen.

We should not however overlook the observations of the lecturer which immediately preceded these examples, inasmuch as they are explanatory of his system, and were in substance nearly as follows: "I am aware," he said, "that as the Drama represents 'the very age and body of the time, its form and pressure,' all the passions may be called dramatic; but as some of them only are oratorical, and the rest purely dramatic, it appears requisite to distribute them into the two classes I have adopted. By dramatic passion I mean selfish,—and by oratorical, I wish to be understood, social feeling. Man is urged into action by selfish and social feelings. If the one be necessary to his individual preservation, the other may be considered requisite to engage him into vigorous and laborious services to his friends, his country, and his whole species. Compassion will engage him to succour the distressed, even with his private loss or danger; an abhorrence of the unjust, and commiseration with the injured, a sense of virtue and honour, can make us despise labour, expense, wounds, and even death. What is properly understood by social feeling constitutes the genuine principle of true, unsophisticated oratory. 'Social feeling,' says Dr. Hutcheson, 'is fixed humanity; it is such a desire of the good of all to whom our influence can extend, as uniformly excites us to every act of beneficence, and makes us careful of informing ourselves rightly concerning the truest methods of promoting their interests.' That social appeals are oratorical, and that an unqualified effusion of pride is *not* oratorical, no one would doubt who saw them exemplified: an audience would hardly be influenced in favour of a speaker who displayed before them the selfish passion of hatred, as actuating his *own* mind. At the same time, could we listen to the detail of the accumulated crimes of Piso, as enumerated and *denounced* by Cicero, we should join with the orator, and participate in his feeling.

"From what has been advanced, it may be readily understood,

stood, that when I speak of dramatic and oratorical passions, the vulgar acceptation of the term 'theatrical' bears no relation to either class. This acceptation seems to imply an idea of censure: but I am of opinion with Mr. Walker, that it is a stale trick to depreciate what from indolence, want of taste, or other incapacity, ourselves cannot attain; so that, in many instances, calling a spirited and efficient pronunciation and delivery *theatrical*, is but an artful method of glossing over or excusing our own inability of speaking with becoming force and energy."

For our own parts, we are inclined to think that those persons who apply the term to whatever appears to them forced, or out of character, have the same narrow notion of *acting* with Partridge, at the representation of Hamlet. He thought "the innocent-faced" man who performed Claudius the best actor: "for (said he) Mr. Garrick does only just as I or any body else would do in his situation:—but the king for my money; he speaks all his words distinctly, and half as loud again as t'other. *All the world may see he is an actor.*"

"If the man who addresses an assembly," continued Mr. Wright, "imitate passion when he should only describe it, or describe when he should but narrate, borrowing a phrase from the theatre, I would certainly call him 'the mere actor;' because his efforts and manner carry with them the air of fiction.

"Should the word 'theatrical' be employed to signify 'a dissimulation of real sentiments, or the affectation of adopting the opinions and language of another,' then may the term be forcible and correct; but I am inclined to believe that few who have employed it (any more than Mr. Walpole, who had cause to remember applying it to Lord Chatham,) have intended to convey by it other than a censure and reproach." It is certainly extremely inconsiderate, if not ungenerous, to confound under one term the clumsy caricature of an affected speaker, with the chaste delivery and accurate delineation of nature, sought and so justly admired on the stage at the present day.

Adhering, with some deviations and refinements, to the system formerly promulgated by Aaron Hill, Mr. Wright commented with considerable discrimination and success on the various examples produced by him. These it would be useless merely to enumerate; and to do more would be incompatible with the bounds prescribed us: they occupied the greater part of this and the succeeding lecture. We shall content ourselves with quoting an additional observation on the

the oratorical character. "Correspondent with the authority of a public speaker should be his air or character of expression. However we may be inclined to doubt the sincerity of strangers, the intention of a parent can never be disputed. An orator, then, whose character has not only been irreproachable, but whose ability has been proved, and whose moral principle has been displayed by active social love, may undoubtedly assume the authority of a parent, and enforce every emotion of his mind with the earnestness of a father prescribing salutary rules for the conduct and government of his children."

Having shown how the countenance and voice are affected in expressing some of the principal passions, Mr. Wright proceeded, in his eighth lecture, to speak of the mechanical means through which the various alterations of appearance and sound may be accomplished.

"Taking it as already granted, that when the body is disposed to the appearance of any one passion, by a mechanical effort of the will the mind becomes sensible of alteration, and feels the particular passion;" the lecturer enforced, as indispensably necessary, a ready and familiar acquaintance with the various circumstances of countenance and gesture, connected with the passions and their modifications; advising, at the same time, a frequent recurrence to the mirror, and a comparison, by the student, of the faithful transcript of his look with his actual feelings. Much advantage he conceived attainable from an acquaintance with the writings of Sterne; whose readers indeed may, at times, almost fancy themselves intent on a masterly painting, rather than on a printed page.

The instrumental powers of voice next engaged the attention of the lecturer; upon which he noticed at considerable length the best means of rendering them flexible, sonorous, strong, melodious, and swelling. "The voice," observed he, "like every other faculty of the body, may be improved by judicious exercise. This too, like the sight and other organic powers, may be so exerted as to destroy rather than strengthen it. Every one in familiar conversation may be said to have a key note, one which he more ordinarily employs than any in his compass. This sound should be improved by repeated exertion, being careful to make the tone as sonorous as possible. This, with the assistance occasionally of a musical instrument, while pronouncing any given passage in a monotone will be found of considerable service. The two inflexions should be thoroughly practised; a theme should then be selected, in which

 the

the modulation does not vary: this, and an accurate observance of the proper inflexion, should be studied carefully before any further attempt is made. This being sufficiently practised, it may be said that one octave of the student's voice is in tune. The same course should be pursued one note higher, with a like observance of inflexion; and so on till the whole is practised upwards. The lower or under voices should then be proceeded with, using the same inflexions till completed.

"To express our common ideas, we make use, then, of that key from which we tune our whole compass. This is the key in which our voice is susceptible of the greatest variety of modulation. It is on this that all our efforts to improve the voice should be directed.

"The situation," the lecturer continued, "of a public speaker, with relation to his audience, as regards compass and variety of voice, is one exacting the nicest art and discrimination. He would wish to address a whole assembly with as much apparent ease to himself, and pleasure to his audience, as though it were composed but of one person.

"A public speaker, in addressing himself to his auditory, who meet either to be informed or amused, should adopt, to convey his sentiments, his ordinary and familiar tone of voice. He should endeavour to be heard in this familiar tone and facility of utterance by the most distant person, without offending the ear of the closest: in a word, a public speaker should be solicitous that the tones of his voice should be sufficiently audible, distinct, and natural, to every person in the whole assembly."

To attain these important objects the lecturer afforded many practical rules, to offer any abridgement of which would be doing them injustice: dismissing, then, the subject, he passed on to consider the "Genera of Causes," the distribution of which into Demonstrative, Deliberative, and Judicial, is said to have been invented by Aristotle. The definitions and elucidations of these genera occupied the remainder of the eighth lecture.

"A mixed assembly, a concourse of men, women, and children, are not insusceptible to logical induction, or to the effect occasioned by the beauties of rhetorical refinement; to excite their attention to just notions and feelings, to stimulate the actions of their wills, demand the whole strength of oratory. *Particular* assemblies may be more refined; and these assemblies we recognize in the oratory of the Bar and the Senate. The former accords with that genus termed by the ancients Judicial; controversy arising

from

from what is past, and contesting certain points, just or not, according to the letter and spirit of the law. The latter is said to belong peculiarly to the Deliberative genus: but, connected with the senate, the object of this genus being to contend for new decrees of state, it must appear that the Judicial also is inseparable from certain objects of senatorial eloquence.

"The Demonstrative, by which the ancients understood 'commendation and censure,' can hardly perhaps be called a genus: connected, however, with the Deliberative or the Judicial, the talent of praising or dispraising must add considerable strength and importance to the arguments, and also assist towards conviction. As virtue and authority is to oratory in general, so is the demonstrative to a judicial or deliberative cause in particular."

The ninth and concluding lecture consisted chiefly of a peroration of the present and former courses. Of the progress of both we have endeavoured to give the reader a succinct account. In conclusion, Mr. Wright delivered a high and, we cordially believe, a merited eulogium on the Society whose members he had addressed; from whom the lecturer gratefully acknowledged to have received much liberal indulgence and attention; congratulating the Society on their happiness in possessing a President who had fostered it in its earliest infancy, and whose learning, talents, and amenity of character would be honourable to any association.

GEOLOGICAL SOCIETY.

At a meeting of this Society on January 1, 1813, (the President in the chair,) the reading of Mr. Philip's paper "on the Veins of Cornwall" was concluded.

The metalliferous veins of the Herland and Drannack mines run E by N and W by S, and the cross courses run N by W and S by E. The rock or country which they traverse is schist, in some places so hard as to require being blasted. The width of most of the metalliferous veins varies from two inches to six inches: whenever exceeding this latter measure, they have been found soon after to divide and pass away in mere strings. A contre or oblique vein traverses these mines in a direction W by N and E by S, varying in width from one to three feet. Near the surface it was found to abound in blende and iron pyrites, but lower down afforded large quantities of copper ore. Whenever it intersected the metalliferous veins, the place of junction formed one lode for about eight fathoms in length and three or four in width. The contre was heaved by

by the cross courses, and these latter at the place of intersection are found to be not only enlarged, but impregnated with ore. The contents of the cross courses are clay, quartz, or a mixture of both. It was in one of these cross courses, at the place of its junction with one of these metalliferous veins, that the celebrated deposit of silver was found mingled with galena, with iron pyrites, with bismuth, cobalt, and wolfram: and these substances were also found in those parts of the vein adjacent to the cross course.

Huel Alfred is in immediate contact with the mines just mentioned, and is, at present, one of the richest and most profitable copper mines that Cornwall can boast of. The great deposit of ore is contained in a contre from 9 to 24 feet wide, which is considered as the continuation of that in Herland mine. The contre traverses a regular east and west vein; and it is remarkable that the ore, abundant as it is, has hitherto been found only in one mass at the depth of 117 fathoms, at the point of junction of the contre and of the vein, giving off a branch 110 fathoms in length, along the eastern part of the same vein.

Another singular circumstance in this mine is, that one of the cross courses is heaved and intersected by an E and W vein.

Since the beginning of 1801, there have been sold about 45,000 tons of copper ore, the produce of Huel Alfred, for the sum of about 350,000*l.* of which the profit divided among the adventurers has amounted to about 120,000*l.*

January 15th. (The President in the chair.) A paper by Wm. Conybeare, Esq. M. G. S. "On the Origin of a remarkable Class of Organic Impressions occurring in Nodules of Flint," was read.

This paper, which is chiefly occupied by detailed explanations of the drawings by which it is accompanied, relates to a class of substances thus characterized by Mr. Parkinson, in the second volume of his work on Organic Remains:

"Small round compressed bodies not exceeding the eighth of an inch in their longest diameter, and horizontally disposed, are connected by processes nearly of the fineness of a hair, which pass from different parts of each of these bodies, and are attached to the surrounding ones; the whole of these bodies being thus held in connexion." p. 75.

Mr. Parkinson conjectures that the formation of these bodies has been the work of some polype, similar to those by which the common zoöphytes have been constructed, and therefore classes them among fossil corals of unknown genera.

genera. He observes however at the same time, that his reason for this arrangement is only a very slight analogy, as the objects in question differ materially from every known zoöphyte recent or fossil.

Mr. Conybeare having been so fortunate as to obtain several specimens of this fossil in a much better state of preservation than usual, shows clearly that they occur between the bony plates of a large bivalve shell, the ostreopennite of Walch; and, in a similar situation, in fragments of a striated shell, one of the patellites of Da Costa, which more probably, however, belongs to the genus *Ostrea*. Similar substances have also been observed on the surface of a cast of the Echinus. The matter of which these bodies are composed is flint; and they are supposed by Mr. Conybeare to be casts of the cells of some minute parasitical insect inhabiting the substance of the shells of certain species of the testaceous Molluscæ, and probably deriving hence its nutriment either in whole or in part.

The Anniversary Meeting of the Society for the election of Officers, &c. was held on Friday, the 5th of February, when the following members were elected:

OFFICERS.

President.

The Hon. Henry Grey Bennet, M.P. F.R.S.

Vice-Presidents.

Sir Abraham Hume, Bart. M.P. F.R. and L.S.
Robert Ferguson, Esq. F.R.S.
Sir Henry Englefield, Bart. F.R. and L.S.
John MacCulloch, M.D. F.L.S.

Treasurers.

William Hasledine Pepys, Esq. F.R.S.
Samuel Woods, Esq.

Secretaries.

Leonard Horner, Esq. | Arthur Aikin, Esq.

Foreign Secretary.

Samuel Solly, Esq. F.R.S.

COUNCIL.

The Council consists of the above Officers of the Society, and of twelve other Ordinary Members.

The Ordinary Members for the present year are

Alexander Apsley, Esq. | Alexander Jaffray, Esq.

William

William Blake, Esq. F.R.S.	James Laird, M.D.
J. G. Children, Esq. F.R. and L.S.	James Parkinson, Esq.
Samuel Davis, Esq. F.R.S.	Smithson Tennant, Esq. F.R.S.
James Franck, M.D.	Hen. Warburton, Esq. F.R.S.
G. B. Greenough, Esq. F.R. and L.S.	Wm. Hyde Wollaston, M.D. Sec. R.S.

Keeper of the Museum and Draughtsman.

Mr. Thomas Webster.

Feb. 19th. The President in the chair. John Bostock, M.D. and Thomas Stewart Trail, M.D. of Liverpool, were elected members of the Society.

A paper by John Taylor, Esq. M.G.S. on the Œconomy of the Mines of Cornwall and Devon was read.

The subjects treated of in this paper are:

I. The nature of the agreements between the owner of the soil and the mine adventurers.

II. The arrangements between the partners or adventurers themselves, and the system of control and management appointed by them.

III. The mode of employing and paying the miners and workmen, in use among the agents of the principal concerns.

IV. The purchase of materials for carrying on the undertaking.

V. The sale of the ores from the mine adventurers to the smelting companies.

1. The regulations of the *stannary laws* refer only to mines of tin: hence the search after and working lodes of copper, lead, and other metals, is left open to such conditions as the adventurers and the lord of the soil can mutually agree upon. In general the lord grants a lease for twenty-one years, determinable, however, at any time, on his part, if the mine should not be effectually worked. In return he requires a certain proportion, varying according to circumstances from an eighth to a thirty-second part of the ore, to be delivered to him on the mine in a merchantable state, or its value in money. He stipulates for a power of inspecting the works at all times, and binds the adventurers to maintain and have at any determination of their grant all the shafts, adits, and levels perfect, and in good condition as to timbering.

2. The adventurers divide the whole concern into sixty-four shares, which they share among themselves, and those who are allowed to join them, in various proportions. At the

the end of every two or three months a general meeting of the adventurers is summoned, a statement of the accounts is laid before them, and the profit or loss is distributed to each according to the amount of his shares.

The general detail of management is usually delegated to one person, under whom are subordinate superintendants called captains, selected from among the working miners for their skill and character.

3. The work of the mines, both on the surface and below ground, is almost universally contracted for by the piece, at a kind of public auction held at the end of every two months, an accurate survey and measurement of the whole being previously taken by the captains. The lowest bidder has the *set*; and in order to execute it, he associates to himself from one to eleven men, women, or children, according to the nature of the work. An account is then opened between the principal captain and the contractor, in which this latter is credited with all the tools, candles, gunpowder, and subsistence money required by him and his gang during the term; at the end of which the tools and articles not used are returned; the account is balanced, and the gain or loss upon the contract is declared to the persons interested.

4. If materials for the use of the mine are purchased from those holders of shares who deal in the articles wanted (as is not unusual), great vigilance is required in the other proportions to check the natural temptations to charge exorbitant prices, and to encourage a wasteful consumption.

5. The smelting companies for copper have seldom any share of the mines. There are about fifteen copper companies, all of which have agents and assay officers in Cornwall, though the smelting itself is carried on at Swansey. A weekly meeting is advertised to be held at some place near the principal mines, where the ores on hand, allotted into suitable parcels (the produce of one mine being carefully kept separate from that of another), are offered for sale. Previous to the day of sale, the persons intending to purchase attend at the mines for the purpose of taking samples, which are immediately put into the hands of the assay masters. The agents for the smelting companies being thus furnished with the requisite information, attend at the meeting, and each hands up to the chairman a note or ticket, containing the price per ton which he is disposed to give. The chairman then reads aloud the various offers, and the highest is declared the purchaser.

EDINBURGH INSTITUTE.

At a general meeting of the members of this Institution, on Tuesday, February 2, for hearing communications on subjects connected with science, literature, and the arts, Dr. Millar in the chair, the following papers were received:

1. Account of a new method of ascertaining the quantity of spirituous liquors by *weight*, proposed as a substitute for measurement;—communicated by Mrs. Lovi.

The patent aerometrical beads were invented by Mrs. Lovi, the patentee, some years ago. They consist of small hollow glass balls, with short stems, blown of different specific gravities, from 800 to 1800, and corresponding to the even numbers 800, 802, 804, &c. each bead having the specific gravity it denotes engraven on the upper part of its surface. Those used for spirituous liquors are 30 in number, and extend from 890 to 948. In using them, a small quantity of the liquid, whose specific gravity is to be ascertained, is put into a phial, and several beads are successively immersed in it, till one is found that remains suspended below the surface of the liquid, without sinking to the bottom. The number on this bead indicates the specific gravity of the liquid. A thermometer accompanies the beads, for taking the temperature of the liquid; and the correction necessary to be made when this exceeds or falls below 60°, the standard temperature, is exhibited by a sliding rule.

These beads exhibit the specific gravity of liquids much more correctly than the common hydrometers do, and they are not liable to go out of order. Except breaking, they are not exposed to any accident that can in the least degree impair their accuracy; whereas it is well known that the common metallic hydrometer is injured by the chemical action of the air, and the fluids it is used to measure; has its bulk diminished by a partial depression of its surface, or its weight increased by admitting portions of the liquid into its inside; and, even in its most perfect state, is an instrument upon which little dependence can be placed.

The specific gravity of spirits being determined with facility by the beads, the quantity in gallons, or measures of any other denomination, may be found by weight with the utmost certainty. For, since the weight of a cubic foot of water, and the number of cubic inches in a wine gallon, have been carefully ascertained, the specific gravity of any other liquid will enable us to ascertain the weight of a wine gallon

gallon of that liquid; and, in the same way, the weight of any larger quantity may be found. On the other hand, the weight and specific gravity being given, it is equally easy to find the number of gallons. Such a method would evidently be less convenient than measurement, when applied to small quantities of liquor; but, on a large scale, it would certainly be found greatly preferable. A cask holding 180 gallons, for instance, when filled with a 5-gallon measure, according to the present practice of spirit-dealers, requires 36 distinct manipulations, each of which ought to be conducted with considerable care and attention. But such a cask might be filled with equal accuracy, and less labour, and in a much shorter time, by weighing the cask, first when empty, and afterwards when full: the difference of these weights would of course be the weight of the spirits, from which the bulk or measured quantity might be obtained, as already stated. Such is the method proposed. It has been already put in practice by a few individuals, and would probably have been generally adopted, had it not involved calculations too laborious for practical men,—a disadvantage which the correct and ample tables Mrs. Lovi has now in the press will completely obviate.

It has already been stated, that this method is more expeditious than common measurement, and it may be added that it is more œconomical; as, by substituting one operation for a great number, it avoids the waste occasioned by the frequent mechanical agitation of the spirits, and the loss that constantly attends a series of small operations. Superior accuracy, however, will be found to be its greatest recommendation. This it owes to several circumstances: 1. Because measures are seldom made with the same accuracy as weights; 2. Because they have their capacities changed by variations of temperature, or by accidents, and are much more acted upon by air and moisture; and lastly, Because they do not show excess or deficiency in the quantity of the substance measured, with so much precision.

2. Account of an improved crane, invented by Mr. Kerr, mathematical instrument maker.

The base of this machine rests upon cones, by which the horizontal motion is effected; and the arm is lengthened or shortened by a peculiar contrivance, of which it would be difficult to convey an adequate idea without a diagram. This improvement obviates the difficulties which have long been experienced in the use of cranes. With its assistance, a weight may be laid down, with the greatest accuracy, on any point *within the area* of the circle described by the extremity

extremity of the beam, excepting where the machinery stands. It may be used with much advantage in loading or unloading ships, as it never interferes with their *rigging*. It may be also extremely useful in building, where it is often necessary to place large stones on particular spots, which is very difficult to accomplish with cranes on the common construction. The whole apparatus is extremely simple, and attended with little expense. A model of this improved crane was exhibited in presence of the meeting.

3. Notice respecting some Experiments on Alcohol. By Mr. Hutton.

For the contents of this notice, see Mr. Hutton's paper inserted at length in the present number, p. 130.

XXIV. *Intelligence and Miscellaneous Articles.*

ARTIFICIAL COLD.

The method employed by Professor Lesslie to produce intense cold, by placing water over an open vessel containing sulphuric acid, and subjecting both, under the receiver of a powerful air-pump, to quick exhaustion, is already known to the public. The rapid vaporization from the water quickly lowers the temperature of the residue, which is ultimately converted into ice. By investing the bulb of a mercurial thermometer with a thin coat of ice, and subjecting this to exhaustion over sulphuric acid, the Professor has also succeeded in freezing the mercury.

By a communication from Dr. Marcet to Mr. Nicholson * we learn that that gentleman has effected the congelation of mercury by simply substituting the evaporation of ether for that of water under the receiver of an air-pump. For convenience, the graduated stem of the thermometer should pass through a collar of leather in the plate that covers the receiver. The bulb (which should descend a few inches into the receiver) wrapped in a little cotton wool, or in a little bag of fine fleecy hosiery, being dipped in ether, the plate is then placed over the receiver, which is exhausted as quickly as possible. In two or three minutes the temperature is reduced to about 45° below 0, when the mercury rapidly sinks and is speedily congealed. This experiment succeeds whether sulphuric acid be inclosed in the receiver or not, especially if the temperature of the apartment be as low as 40°; but it is more certain when the acid is present.

* Nicholson's Journal, vol. xxxiv. p. 119.

present. When the surrounding temperature is unfavourable, the success of the experiment may be facilitated by first dipping the clothed bulb in water, and, after freezing this by means of the air pump, pouring a few drops of ether upon it, and again exposing it to exhaustion.

In another part of the present number will be found an interesting notice by Mr. Hutton of Edinburgh respecting some experiments on alcohol, effected by means of artificial cold. The author, in giving his results, has been silent respecting the method employed for reducing the temperature of the alcohol; but Dr. Marcet's process, described above, would certainly answer the purpose. Indeed many new results, obtained by means of this powerful agent, may be speedily expected.

DR. WOLLASTON'S CHRYOPHORUS.

In a paper read some weeks ago before the Royal Society, Dr. Wollaston describes a new instrument, to which he has given the above name. It consists of a tube, terminated at each end by a ball, and bent like the letter U, having one of these half full of water, the other empty, and the whole exhausted of air. If the empty ball be plunged into a mixture of salt and snow, the water in the other ball will be frozen in a few minutes, though several inches, or even some feet, distant from the cold mixture.

In the communication alluded to in the preceding notice, Dr. Marcet states, that, by a process similar to that described for the congelation of mercury, the water in Dr. Wollaston's instrument may be frozen without any cooling mixture in less than a minute, and with a pump of very moderate power.

SUGAR FROM STARCH.

In the preceding volume of the Philosophical Magazine some account was given of this process, the discovery of M. Kirchoff of the academy of Petersburgh. M. Kirchoff employs sulphuric acid, one part diluted with 200 parts of water, which is made to boil in a well-tinned copper vessel. Starch 100 parts, mixed with 200 parts of water and passed through a sieve, is then gradually and in small quantities mixed with the boiling diluted acid; and the whole is kept in a state of ebullition for 36 hours, water being added for what is evaporated: some powdered charcoal is then added, and, lastly, chalk sufficient to saturate the acid. It is then filtered through a cloth, and afterwards evaporated gently to the state of a syrup, and set aside for crystallization, which takes place in three or four days.

M. Vogel

M. Vogel has repeated M. Kirchoff's experiment *, using 2 parts of sulphuric acid to 100 of starch. After clarifying, when cold, with charcoal and chalk, he evaporated nearly to a syrup, set it by to cool that more of the sulphate of lime might fall down, decanted off the clear liquid, and then finished the evaporation. When he used a silver basin in place of a tinned copper vessel (which is acted on by the acid), the sugar was sweeter and whiter; but a leaden vessel was found to answer every purpose. From a mean of several experiments, starch appears to yield its own weight of syrup. The syrup mixed with yeast yielded carbonic acid by fermentation, and by distillation a notable quantity of alcohol.

The fecula of potatoes, substituted for starch, gave also a very saccharine gummy syrup: the syrup of starch always contains gum, the proportion of which varies according to the time of boiling and the quantity of acid employed. The gum, separated by means of alcohol, seemed in no respect to differ from gum arabic, except in not forming mucous acid with nitric acid.

Sugar of milk, which never ferments, acquired that property when treated with sulphuric acid, and yielded alcohol.

Dr. Tuthill has also converted the fecula of potatoes into saccharine matter †. From $1\frac{1}{2}$ pound of this fecula (the produce of $8\frac{3}{4}$ pounds of potatoes), treated with six pints of distilled water and $\frac{1}{4}$th of an ounce of common sulphuric acid, and clarified with charcoal and chalk, he obtained $1\frac{1}{4}$ pound of a crystalline mass resembling common brown sugar mixed with a little treacle; and from one pound of this mass he obtained by fermentation and distillation two ounces and five eighths by measure of dilute alcohol, of such a gravity as (by calculation from Sir Charles Blagden's experiments) made the produce equal to 14 drams by measure of proof spirit.

SAWING CAST IRON WITH A CARPENTER'S SAW.

M. Dufaud in a letter to M. d'Arcet, director of the ironworks at Montalaire, published in the eighty-second volume of *Ann. de Chim.* announces that he has succeeded in sawing cast iron with a carpenter's saw, and that all that is necessary to ensure its being sawn as easily and in the same space of time as dry wood, is that the iron be heated to a cherry red. For heating the iron a furnace is preferable to a forge fire, as the temperature is thus rendered more uniform

* *Annales de Chimie*, vol. lxxxii.

† Nicholson's Journal, vol. xxxiii. p. 319.

throughout

throughout the mass. The iron should be so placed as to have a firm bearing every where, except where the saw is to pass, to prevent any part from being torn off by the saw; and the iron should be cut briskly, using the whole length of the saw, the teeth of which should be set fine. By this simple method not only plates but mill gudgeons, and even anvils, have been cut with great facility. When the piece to be cut is large, two saws should be employed, for the convenience of using and cooling them alternately: the saws receive little or no injury. This useful process, though not generally known, is not new: several years ago M. Pictet observed a workman saw a hot cast-iron pipe in the workshop of Mr. Paul of Geneva.

On Saturday the 20th of February this useful process was tried in the presence of several gentlemen at the iron foundry of Mr. Williams, in Waterford, and the success of the experiment was complete. The operation was repeated several times, and always with facility. The iron, as stated above, should be heated to a cherry red, and the saw need only be selected according to the fineness of the pieces into which the metal is to be cut. The operation is perfectly easy, and the saw remains uninjured.

HEATING BUILDINGS BY STEAM.

This beneficial practice is every day coming into more general use. Not only are large manufactories, as cotton-mills, now rendered comfortably warm in this manner, but churches and less extensive buildings. Some time ago a plan was presented by Mr. Robertson Buchannan, civil engineer, to the magistrates of Aberdeen, for heating one of the churches in that city, (a Gothic building we believe,) which has since been executed, and gives perfect satisfaction. The fuel is put to the boiler on Saturday evening, and is continued till the congregation meets for the afternoon service on Sunday. At the end of January the steam heat brought the temperature of the place to 46° or 48°, which was increased by the presence of the congregation about 4° or 6° higher. The printing-office of the Glasgow Chronicle has for some time been comfortably and œconomically heated by Mr. Buchannan's arrangement of steam tubes.

A recent number of the Gazette de Santé contains the following account of a remarkable recovery from the effects of poisonous mushrooms. A boy ten years of age, having incautiously eaten some mushrooms which he had picked

up

up in a wood, was almost immediately taken ill. He was conveyed to his parents in a dying state, and, from certain circumstances, four days elapsed before he was visited by a medical practitioner. A Dr. Dufour having been sent for, he found the child in the following state: Countenance of a ghastly paleness; clammy sweat over the body; eyes open and fixed, exhibiting the opaque cornea only; belly flat and hard; the jaws spasmodically closed, so as to prevent all food from being swallowed; the motion of the heart scarcely perceptible. Dr. Dufour immediately broke two of the front teeth, and administered through the aperture a mixture of sulphuric ether and syrup of orange flowers: the body of the patient was then placed among the dried leaves of tansy, dulcamara, jasquiama; and the belly was rubbed with oil of chamomile, camphor, alcohol, and ammonia, mixed up together: every thing was done with a view to heat the patient. This mode of treatment had the desired success; and the child, after swallowing about an ounce of ether, and the same quantity of syrup, completely recovered.

M. Alexander Kis, of Pest in Hungary, has recently invented an universal alphabet, or species of pasigraphy applicable to all languages. At a public exhibition before the members of the various learned bodies at Pest, M. Kis made an application of his invention to the Lord's prayer, which was read in Greek and English by a person present, and immediately committed to writing with the new alphabet, so as to be read with facility by every person present.

Mr. Bakewell will commence a Course of Geological Lectures in March at Willis's Rooms, King-Street, St. James's; designed to illustrate the Geology and Mineralogy of England, and particularly intended to direct the attention of landed proprietors to the neglected mineral treasures on their own estates. Mr. Bakewell also intends shortly to publish in one volume octavo a Work entitled Outlines of Geology, with Observations on the Geology of England.

Meteorological Observations made at Clapton from January 17 to February 11, 1813.

Jan. 17.—Cloudy and cold wind. SE.

Jan. 18.—Cloudy and raw wind. SE.

Jan. 19.—Cold and cloudy; the range of the thermometer is not above two degrees, nor has it been for many days much more.

Jan. 20.—Cold cloudy day; wind E. The range of the thermometer was only 3°.

Jan.

Jan. 21.—Cold east wind and cloudy, some intervals of star-light by night, afterwards a little snow fell.

Jan. 22.—Cold and cloudy. SE—E.

Jan. 23.—Cold, windy, and cloudy. E–NE. Snow at night.

Jan. 24.—Cold, fine in the middle of the day; stormy wind from SE.

Jan. 25.—Clear in the morning, cloudy afternoon; wind easterly.

Jan. 26.—Cloudy and snow.

Jan. 27.—Cloudy, very high barometer. SE—NE.

Jan. 28.—Fair morning, sudden increase of fogginess at times; fine crimson and gold at sunset. Wind N. and calm; clear night.

Jan. 29.—Clear morning, clouds of indistinct character followed afterwards by general cloudiness at night. Wind E.

Jan. 30.—Cold, raw, cloudy, unpleasant day, and damper than hitherto. SE—N.

Jan. 31.—Cloudy day, fair and star-light at night. Wind N. and NE.

Feb. 1.—Damp cloudy day, and very unpleasant showers of rain at night. Wind N.

Feb. 2.—Damp overcast day, with some small rain about noon. Wind N—SE.

Feb. 3.—Fine day, large elevated masses of *cumulus*, and almost *cumulostratus*. Cloudy night.

Feb. 4.—Fine day, *cumulus, cirrus,* &c. hazy horizon: developments of haze in the air; some rain, fair afternoon.

Feb. 5.—Some rain; fair afternoon.

Feb. 6.—Fair day, *cirrus* varying towards *cirrocumulus* above, loose *cumuli* flying along in NW. wind below, afterwards wind SW.

Feb. 7.—Gentle showers in the morning; afterwards they were harder; the different modifications in different altitudes as usual.

Feb. 8.—Cloudy, misty, and windy, with some small rain; wind SSW—S. and very high by night.

Feb. 9.—Windy; clouds of flimsy texture, in two or three different altitudes; a hard thunder shower with high wind about two P.M. Wind westerly.

Feb. 10.—Fine day, various clouds; clear night.

Feb. 11.—Fair day, *cirrus, cirrocumulus,* and *cumulus,* with haze; between six and seven P.M. a lunar *halo* of about 46° diameter; no visible and definite cloud; barometer 29°28 and falling, thermometer 38°; wind westerly.

Clapton, Feb. 12, 1813. THOMAS FORSTER.

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For February 1813.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Jan. 27	34	37	29	30·40	6	Cloudy
28	26	33	28	·38	10	Fair
29	22	32	29	·36	10	Cloudy
30	31	35	32	·40	5	Cloudy
31	38	45	40	·39	0	Cloudy
Feb. 1	40	42	37	·21	0	Cloudy
2	36	36	36	·20	0	Foggy
3	37	42	35	·35	10	Fair
4	36	42	37	·40	12	Foggy
5	34	43	36	·10	10	Fair
6	40	47	37	29·75	16	Fair
7	40	47	43	·80	10	Showery
8	43	49	42	·52	24	Showery
9	40	46	37	·50	0	Storms with
10	35	47	36	·82	27	Fair [thunder
11	42	48	40	30·00	24	Fair
12	46	50	42	29·68	15	Stormy
13	45	51	43	·20	12	Stormy
14	47	50	46	·15	0	Rain
15	43	51	47	·12	10	Stormy
16	40	47	43	·45	27	Fair
17	44	52	46	·12	0	Rain
18	47	53	50	·50	28	Fair
19	46	54	51	·70	20	Stormy
20	51	56	47	·82	36	Fair
21	47	56	50	·78	27	Cloudy
22	51	54	46	·85	26	Stormy
23	47	47	38	·90	0	Rain
24	36	47	33	30·04	32	Fair

N.B. The Barometer's height is taken at one o'clock.

XXV. *Account of the late Earthquake at the Caraccas**.

THE earthquake which took place last year at the Caraccas, and laid waste the fine city of that name, besides a great many others in this rich and extensive province, has been but superficially described in the newspapers in which I have seen it mentioned. The extraordinary convulsion has not (December 1812) as yet ceased; it has already caused, and may still occasion, so many calamities, that it deserves to be more particularly laid before the public.

On the 26th of March 1812, at five o'clock in the afternoon, the first commotion took place. The air was calm, the heat excessive: nothing preceded or announced such a catastrophe. A shaking was first perceived, strong enough to set the bells of the churches a-ringing: it lasted about six seconds, and was followed by an interval of ten or twelve seconds, during which the earth exhibited an undulation similar to the motion of the sea in a calm: the crisis was then supposed to have passed; but immediately extraordinary subterraneous noises were heard, and electrical discharges infinitely stronger than atmospheric thunder; the earth was agitated with a quickness which cannot be described, and seemed to boil like water when subjected to the heat of a very strong fire; there was then a perpendicular rumbling or *strepitus* for about three or four seconds, followed by agitations in an opposite direction from north to south, and from east to west, for three or four seconds also. This short but awful period was sufficient to turn the whole city of Caraccas topsy-turvy, with upwards of thirty towns, and the country houses and numerous establishments spread over the surface of that delightful province! In an instant all was destroyed to an extent of 300 miles, and 80,000 inhabitants ceased to live, while thousands were dreadfully wounded.

The city of Caraccas, placed at the foot of the declivity of the highest mountain, called La Silla, and on the margin of an immense plain through which several rivers flowed, was considerably elevated above the level of the sea, and always enjoyed a cool and agreeable temperature. The 26th of March (being Good Friday) had attracted all the inha-

* This interesting narrative is the production of a French gentleman, who has resided may years at the Caraccas, and was an eye-witness to the scenes which he describes. He was taken prisoner, on his return to France on board the American ship Dolphin, by Capt. Malcolm of the Rhin frigate. To the latter gentleman our readers are indebted for the publication of the narrative.—EDIT.

bitants to the churches of the city which were destroyed; thus serving for their tombs: the churches of La Trinidad and Alta Gracia, which were in the more immediate vicinity of the mountain, experienced more forcibly the effects of the extraordinary commotion; for although originally upwards of 150 feet high, no part of their ruins exceeded five or six feet in height; and some idea may be formed of the violence of the shock which overturned these stupendous edifices, when it is recollected that they were supported by columns and pilasters exceeding thirty or forty feet in circumference, and of which scarcely a vestige remained.

A superb range of barracks two stories high, capable of containing 4000 men, and serving as a depôt for the artillery, shared the same ruin: a regiment of the line, in the act of marching to join in a religious procession, was almost wholly swallowed up; a few men only being left alive.

It is impossible to paint the terror and desolation which this catastrophe occasioned: disorder, confusion, despair, misery, and fanaticism were at their height. At first every person fled as well as they were able, prostrating themselves to supplicate heaven for mercy; in this state the individuals who escaped death, mutilated or wounded, covered with dust, their clothes torn, and carrying in their arms their children, or the sick and wounded, presented a most heartrending spectacle. After the first moments of terror, in which self-preservation made every other consideration give way, the most painful recollections agitated those who had escaped; every one with distracted anxiety sought for a relation or a friend, and inquired for them with looks of terror and affright: among the bloody and desolate ruins, those who remained of the unfortunate population were seen endeavouring to dig up, without other instrument than their weak and trembling hands, the living and the dead who were covered by the fragments: every one ran to and fro over this vast burial-place, throwing themselves occasionally on the rubbish, and listening with an attentive ear to the groans of the unfortunate whose lives were preserved, although shut up, perhaps irrecoverably, in the very buildings where they had enjoyed tranquillity and happiness but a few minutes before.

The remainder of the day and the whole of the night were devoted to this interesting and pious occupation. Next day, it was necessary to perform the last offices to the dead, but it was impossible to bestow on them the rites of sepulture; instruments and a sufficient number of persons were

were not to be found: in order to avoid the effects of a pestilence, therefore, from an infected atmosphere, the bodies were piled up at different stations and burnt with the timber of the ruins. The first sad moments after the catastrophe were thus spent: other labours, equally if not more distressing, remained to be performed.

Almost all the provisions, furniture, linen, and the usual necessaries of life were destroyed, or had been stolen by the lower class of the populace, or the negroes: every thing was in short wanting. The violence of the earthquake had destroyed the water-pipes, and the rivulets were either dried up, or diverted from their usual course: there was in fact no water near the city; there were no vessels in which to collect it, and it was necessary to travel far off before a quantity sufficient to allay one's thirst was obtained, even by using the hands to carry it to the mouth.

Pressed by thirst and hunger and the want of an asylum, those who possessed country houses fled towards them on foot; but alas! nothing was spared—all was ruin and desolation; and they returned to the city, where they seemed to be less miserable among their companions in misfortune, the silence and solitude of the country apparently adding to the dismal aspect of nature.

The markets were without provisions; the farmers brought none into town; and many, after wandering about in search of food, at length lay down and died of hunger: those who survived obtained sustenance with much difficulty. Had not some cocoa, sugar, and maize been saved, (which were retailed at a most exorbitant price,) more would have perished from hunger than from the effects of the earthquake.

Three thousand wounded of all ranks were collected and placed at first on the banks of a river, under the shade of some trees: but they were absolutely in want of every thing, even the most indispensable requisites: they were abandoned to the medicine of consolation: they were told that they must conform to the decrees of providence, and that every thing was for the best.

During this awful crisis, a judicious observer of mankind might have witnessed a striking exhibition of the manners, character, and principles, by which the Spanish people are regulated in their conduct.

Their extreme insensibility is scarcely credible: I saw fathers of families who had lost five or six children, friends, relations, and their whole property, without shedding a tear; most of them consoling themselves by holding a

 conver-

conversation with an image of the Virgin, or some privileged saint*. Others gaily drowned their sorrow in rum; and all appeared much less grieved at the event, than they would have been at the loss of a process which affected their rank as nobles, or deprived them of their precedence in a public company or at a religious procession.

It is too true, that human beings, naturally superstitious and ungrateful, never so cordially respect their deities or their kings when they are beneficent as when they are severe: the more rigorous they are, the more just and equitable are they esteemed. Such is the lot of mankind! they forget benefits; and governors, in order to acquire the homage which is due to them, must be feared: gratitude and love are sentiments too delicate to be common among mankind.

Good Friday is without doubt the most imposing of the Catholic holidays: it is that which ought to inspire the most pious reflections; but at the Caraccas, as in many other places, on this occasion, the women are occupied with their dress, more anxious perhaps to appear amiable in the sight of men than to worship the supreme Being: they think of nothing but amusement, and they almost forget that Being who does not manifest himself openly. But scarcely had they experienced the earthquake, when they said it was the thunder of Heaven sent to punish the crimes of mortals: their elegant clothes were immediately laid aside; those who had it in their power changed them for coarse garments, by way of showing their penitence: sackcloth, cords, and chains, were substituted for elegant fashions and seductive head-dresses. The ladies now subjected themselves to monastic discipline, and beat without remorse their bosoms, but a short time before adorned with the most costly jewels: many of the gentlemen at the same time forgot their gallantry for fanaticism; and, in order to appease the anger of Heaven, they walked night and day in processions, the body entirely uncovered with the exception of a large girdle, barefooted and with long beards, a cord around their necks to which was frequently attached a large stone, and on their shoulders they sometimes carried a wooden cross 100 or 150 pounds in weight.

In the city and throughout the country there were processions day and night; every mountain was transformed into a Calvary, where the people dying with hunger im-

* The divine Being among the Spaniards seems to be absolutely unknown; they never speak of him: it is the Virgin and the Saints who receive all their homage.

plored

plored the divine mercy, embracing with groans the relics of their tutelar saints.

Every one accused himself of having called down the anger of Heaven, and of having caused the universal calamity: those who could not meet with a priest openly confessed their sins upon the highways, accusing themselves of robberies and murders which they had secretly committed.

In less than two days about 2000 individuals (who perhaps never had any intention of the kind) were married: relations formerly despised or neglected on account of their poverty were now recognised: many unfortunate children, the fruits of an illegitimate intercourse, who had never known father or mother, were now acknowledged and legitimated. At the same time an infinite number of restitutions were made, and law-suits terminated. But notwithstanding all this remorse, a singular and paradoxical spectacle was exhibited to the eyes of the philosopher: while one half of the multitude thus hastened to expiate their offences, the other half, who perhaps never had been guilty of any great crimes before, but possessing an accommodating conscience, profited by the confusion, and with the utmost composure committed every imaginable excess.

In the mean time the shocks from the earthquake continued;—every day and every hour some ruins fell, which had been only shaken by the first commotions. On the 5th of April, at four in the afternoon, there was a shock so violent that several mountains were rent asunder, many inclined from their centre of gravity, and enormous detached rocks were precipitated to the valleys.

From the above hour until nine o'clock next morning the shocks were violent, and so frequent as to admit of an interval of about five minutes only between each; and during these intervals a rumbling subterraneous noise was heard, and the earth was continually agitated.

The succession of these phænomena was not interrupted in the month of December 1812, when I left the place, and those were reckoned the most tranquil days, in which there were only fifteen or twenty shocks! Every thing was destroyed; the ramparts of La Guyra, not less than twenty feet in thickness, were thrown down. As a natural consequence of the opening of the mountains, which are the great reservoirs of water, some rivers were observed to have considerably increased. Many high mountains were rent

 right

right across the centre, and that called La Silla has sunk more than sixty fathoms.

It is difficult to say what will be the close of this dreadful event: it may be hazarded as a conjecture, however, that it will end in the opening up of one or more volcanoes: in the mean time the unfortunate inhabitants of these countries, attached to their native soil, and not wishing to abandon the ashes of their fathers, have with great labour erected rude habitations, in which they await with stoicism and resignation the termination of their calamities.

J. H. S.

XXVI. *Description of a mechanical Instrument to work Addition of Numbers with Accuracy and Dispatch.* By *Mr.* J. Goss, *of Enfield**.

Sir,—About two years ago I resided at Hatherleigh in Devonshire, where I had a day-school, and lodged with people who kept a shop: they had frequent occasion to cast up bills, but having but little knowledge of figures were very liable to make mistakes; they therefore, when a bill was any way long, generally brought it to me, and oftentimes, when I have been out in the town, I have been sent for to come home and cast up a bill; at length I thought if some mechanical contrivance could be invented to cast up bills, it would be of great service to many, or even to all who are in business. I knew that multiplication, division, and many other rules in arithmetic, were often worked mechanically; but addition being in itself so irregular, I was afraid no instrument could be invented to work it. However, by repeatedly considering the subject, I discovered after some time a method of casting up a bill by a slide rule about two feet long and two inches broad; and as I was studying to bring it to greater perfection, an imperfect idea of this addition-wheel sprung up in my mind, which is a much better method than the former: but thinking the experiment would be attended with expense, and after all perhaps be of no advantage to me, it lay dormant in my mind till about last Michaelmas, at which time I came to London, and a friend of mine got me one of the lists of premiums offered by

* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce,* for 1812.——The silver medal of the Society was voted to Mr. Goss for this communication, and the Instrument is preserved in the Society's Repository.

by the Society of Arts, &c. published in the year 1809. After I had read of those honorary and pecuniary rewards which had been given, and were then offered, my desire to obtain some mark of the Society's approbation could not be appeased but by possession, and I was determined to carry my idea into execution; I immediately renewed my study of this instrument with increased application.

Casting up bills is what falls to the lot of most people in business, and many who are moderately clever at it often find it a troublesome task before they can place any dependence on their being right; they have need to cast them up two or three times, and even then have often as many different sums, and therefore frequently find themselves much confused and puzzled in the operation: the instrument of my invention in such cases would be very acceptable: it would take the work from the *mind*, and give it to the *hand*, which would perform it with greater ease, accuracy, and expedition; a person who can only *read* figures, may by this help add up a bill with as much accuracy as a mathematician.

The same day I completed my instrument, I showed it to the people with whom I lodged, who as I have already observed were shop-keepers. I wrote a bill, and desired them to cast it up; I then showed them how to do it by my wheel, and desired them to add up the same bill by it, and see if it was right. They then proceeded, and cast it up right by the wheel, when they discovered that they had made a mistake of one shilling in the row of pence, and two shillings in the row of shillings. They were therefore much pleased with my new contrivance, because it was more true and less troublesome than the common way.

This wheel has four circular rows of figures upon its face. The first row which is nearest the teeth on the circumference, denotes *pence*, the second *shillings*, and the third and fourth denote the *total number of pence or shillings*, &c. Thus, if 64 in the third row should be under the index, if I were casting up pence, I should see in the first row, 4 under or next before the index, and the next red figure passed over by the index would be 5, which signify 5*s*. 4*d*., the red figure or figures nearest the index signifying *shillings*, and the black figure or figures before the index the odd pence. In the second row, the black figure before the index signifies the number of odd shillings, and the next red figure the number of pounds. Before I begin to work, the red figures 360, 180, 9, 15, must always be placed next before the index. I then begin to cast up

up the row of pence. If I should have 5*s.* 4*d.* I set down the 4*d.* under the bill, and bring back the red figures 360, &c. again before the index; then, with the brass handle I move round the wheel 5 divisions, and go on with the row of shillings, &c.

This addition-wheel has cost me much time and thought; but should it be honoured with the approbation of the Society, I shall feel myself much gratified.

I am, sir,

Your humble servant,

No. 4, Wood-Street, Spa-fields, Jan. 21, 1812. J. Goss.

To C. Taylor, M.D. Sec.

Reference to the Engraving of Mr. J. Goss's *Instrument to work the Addition of Numbers in Arithmetic.* Plate V. Fig. 1, 2, 3, 4.

This instrument consists of a brass hoop, fixed to a flat circular plane of wood; this hoop is divided on its upper edge into 180 ratchet or saw-like teeth, and the circle has a number of radii lines of figures upon its face, in divisions corresponding with the teeth; also of a supporting circle, having a fixed index reaching across those lines of figures; and a circular row of 20 divisions, and another of 50, correspondent to the ratchet teeth; and of a brass central index which takes into the teeth, and will turn the ring in one direction only, to one certain place or stop; and then, the numbers on the circle, close to the fixed index, will show the sum total of the different numbers to which it has been turned round, at any number of intervals. Fig. 1. is a plan, showing a portion of the moveable hoop and circle, and the numbers which are upon its face. Fig. 2. is a section of the instrument, answering to the same. Fig. 3. is a plan, on a smaller scale, of the instrument on the under side; and fig. 4. an edge view corresponding with it; the same letters of reference are used in all the figures. AA represents the principal upper or moveable circle, on which some of the numbers are marked; this is attached by a centre pin R to another circle BB, figs. 2, 3, and 4, which is held in the hand when the instrument is used; these two circles turn round freely upon each other, and upon the centre of the upper one a radial lever, or index, CL, is fixed, which has a free motion round the centre pin R. The circle AA has a ring or hoop of brass MM, fixed round its circumference, which is cut into 180 serrated teeth, as shown in fig. 2. The

centre

centre index CL slips over the sloping side of these teeth when moved in one direction; but when moved in the other, its edge *c* catches into the perpendicular sides of the teeth, and carries the circle round with it. EE, fig. 3, are two brass cocks, screwed to the side of the lower circle BB, and projecting from it beyond the circumference M of the upper circle; the ends of them support a flat circular wooden or brass limb, FF, which (as shown in fig. 1.) has other correspondent divisions and figures upon it, over which the index passes: at one end of the limb a wire stop, *b*, is fixed; and when the index is pressed against this, its edge *c* will stand upon the figure 1. of the limb FF, which is numbered on progressively, 1, 2, 3, 4, 5, 6, &c. to 50; which numbers are the same distance apart as the teeth upon the edge of the great circle *A;* so that, by moving the index to any of these numbers, its edge C will have passed over the same number of teeth of the circle, as the number of the limb which it is carried to denotes; but in passing in that direction it slips over the sloping edges of the teeth without moving the circle: now, the edge C having arrived at any intended number, as 19, for instance, the edge of the lever is pressed into the teeth; and being brought back again till it touches the stop *b*, it will have moved the circle A round 19 teeth. At the extreme end of the limb FF, a piece of brass, PP, is fixed, so as to form a reading-index for the numbers on the several circles, which are described on the face of the great circle AA: these are four in number, viz. one for the pence, one for the shillings, and two circles for the pounds: the external circle, which is the pence, is numbered 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, then 1, marked in red, to denote 1 shilling*: then 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, again, and 2, in red, to denote 2 shillings, and so on, up to 180, which will be 15 shillings: the next circle towards the centre is for the pounds, this is numbered 1, 2, 3, 4, 5, 6, &c. up to 19, then one, in red, for one pound: then 19 numbered successively again, and 2, in red, for two pounds, and so on till 9 pounds, which fills the circle, because 9 pounds contain 180 shillings: the third circle towards the centre is for the addition of pounds, or any other whole numbers: the circle therefore is numbered in regular ascending series, from 1 to 180; but to enable the instrument to count higher than 180, the fourth circle is introduced: this begins at 181, and proceeds, by a regular increase, to 360; G, figs. 3 and 4, is a detent,

* Those figures, which in the instrument are marked in *red*, the engraver has distinguished by including them in a small circle.

moveable

moveable on a centre pin attached by a stud H to the lower circle BB, and its tail is pressed by a small spring *h*, which causes it to press constantly upon the under side of the great circle A A, and produces such a friction as prevents the upper circle slipping loosely round; a screw *k*, fig. 3, is fixed in one part of the under side of the lower circle, so that in turning round it intercepts the detent G, and in this position the edge of the index PP is at the zero, or point of commencement of all the numbered circles. In this position the instrument is ready for use, in the following manner: suppose the following sum is to be added up:

£.	s.	d.
23	14	3
18	5	2
12	3	4
47	6	5
21	4	3
12	3	4
134	16	9

Having adjusted the instrument as before described, that is, having brought the circle to the zero, hold the circle BB in the one hand, and take the end L of the lever CL in the other: then move the end of the lever CL till its edge *c* cuts the figure 4 of the limb FF, which is the first figure in the sum: in this movement the index is held up so as not to touch the teeth; but having arrived at the intended figure, it is pressed down into the teeth, and is brought back again (the circle with it) until it touches the stop *b*, when it will have moved the circle, so that 4 stands before the index P on the pence circle; then the index L is carried back again to 3, the second figure of the sum, and returned to its stop, carrying with it three divisions more; it is next moved to 5, and so on, following the pence column till the number 3 at the top is counted; then, examining at the edge of the index PP, it will be found to stand at 9, in the pence circle, and the nearest red figure which has passed by the index will be 1, denoting 1 shilling and 9 pence; therefore 9 must be put down, and 1 carried to the next column: and to recollect this, a small pin, *x*, must be stuck into the hole No. 1, upon the outside of the limb F: the circle is then returned to the zero, which is readily performed by turning it backwards as far as it will go, and the stop *k*, fig. 3, prevents its going further than the right position: the column of shillings is then added up by the same process, taking the numbers 3,

4, 6,

4, 6, 3, 5, 14, by successive steps of the index L: then, on examination of the second or shilling column, 16 will be found beneath the index, and the nearest red figure which it has passed by will be 1, denoting 1 pound 16 shillings: 16 therefore is set down, and the pin x still kept in the same hole to denote that one is carried forwards; the circle is again brought to the zero, by bringing it back as far as it will go; and lastly, the column of pounds is added, in exactly the same manner.

XXVII. *Dissertation on the Paintings of the middle Age, and those called Gothic. Extracted from an unpublished Work on Painting, by M.* PAILLOT DE MONTABERT.

[Continued from p. 44.]

Of the various Schools of the middle Age.—Roman School of the middle Age.—Greek School of Constantinople from the ninth to the sixteenth Century exclusively.—Florentine School of the middle Age.—Venetian School of the middle Age.—Gothic School of the North.

Roman School of the middle Age.

CONSTANTINOPLE for a long period gave laws to Europe in the arts; but in spite of the influence which this school may have had over the painters of Rome, the ancient models, always reviving in this rich capital of the world, presented nourishment too abundant and too wholesome to encourage a preference for the new style of paintings sent from the East, to which they conformed occasionally merely out of condescension. All the artists of Rome, in short, down to the time of Raphael, knew how to profit by the innumerable sculptures and subterranean paintings which were daily discovered in that famous city. There cannot be a doubt, therefore, that the character of this school consisted at all times in a correct style, in clear and expressive pantomimes, forcible and yet agreeable, as well as in draperies of a good taste; and we ought therefore to regard this school as the first preserver of the true ancient style of painting *.

Greek

* All the Christian sarcophagi of Rome are executed exactly in the style of the last sculptures of Paganism: and it is surprising enough to see upon these sepulchral ornaments Moses striking the rock, Jesus entering into Nazareth, or standing in the midst of his apostles, and so many other sacred subjects, similar in the costume and workmanship to the representations

Greek School of Constantinople.

When Byzantium became the residence of Constantine, his favourite city was enriched not only with beautiful monuments brought from Rome, but there were also collected at the same place such objects of the greatest rarity as still existed in ancient Greece. The number of famous statues and pictures, according to historians, was immense; and it is truly astonishing that so many fine models had not perpetuated a race of good artists even in spite of every obstacle. To say nevertheless that there no longer remains any thing of the ancient simplicity, or of the grandeur and dignity so essential to the majesty of the art, would betray an ignorance of the progress of the human mind; and, we may even conjecture, that in the same way as certain emperors at Rome caused either from taste or caprice, the ancient style of the Greek sculpture to be imitated, so also we see certain artists of the present day, from motives which we cannot exactly divine, assume the ancient characters of the schools, and reproduce recollections of the beautiful, in such a way that observers, more than once in this school, must have met with figures full of elegance and simplicity *. In general the characters of the Greek school of Constantinople are gravity, dignity, and even beauty, although indicated by feeble means †.

Florentine School of the middle Age.

I should be inclined to believe that the zeal and enthusiasm which were manifested at Florence for ancient literature, at a period probably anterior to that of the Medici, produced among the artists of that period an inventive and poetical taste, and that it was in this school that the

tions of the ancient mythology. In the same way there is reason to believe that the style of portable pictures painted upon wood, and which have been destroyed by time, was a continuation of the style of the preceding paintings. The Catacombs exhibit proofs of this.

* Witness, for example, the statue of Julian the apostate in the Napoleon Museum, No. 6.

† Those who have not seen the grand Mosaics of the most ancient churches in Rome may consult, *inter alia*, the engravings of Ciampini, and particularly that which represents the Mosaic of Saint Agatha of Ravenna, tom. i. tab. xlv. as well as the engraving tab. liv. and several others in the same work, which will show the noble simplicity of the Greek style of Constantinople. It is worthy of remark, that the artists which Italy attracted from this city were rather Mosaic daubers, than painters properly so called. It is a pity that they should have so frequently repeated the ideas of each other: nevertheless, upon considering their works with attention, we discover more variety than is at first imagined; and they possess this in common with all the ancients, who are very different from each other, when they are considered with care and without prejudice.

qualities

qualities of expression were most frequently to be found. To attain this there was no occasion for the assistance of the Greeks and Romans: the mere bent of the genius of the artists and the study of the passions were sufficient. Hence those painters, who gradually got rid of the ancient maxims in their taste and in their style, gave more animation to their figures. Hence proceeded those expressive and true images which have been subjects of imitation with so many subsequent painters; hence those physiognomies truly natural, and inspired by a sound judgement and feeling heart. In this school therefore may be acquired a great accuracy of delineation, a quality which alone perhaps, when it was properly appreciated, formed the Verrochios, the Michael-Angelos, the Leonardis, and the bold designers who adorned Tuscany, and whose celebrity was such that the whole of the painters of Italy, in studying their designs, were forced to imitate their new taste. May we not suppose also, that this custom of servilely copying individual peculiarities, and this neglect of the ancient models, may have led them to introduce into almost all their subjects the costumes of contemporaries? and perhaps several pictures of that time, in which the costume is Grecian, have been executed by Florentine artists.

Venetian School of the middle Age.

Venice received the arts from the East, and her school of painting was much more influenced by the painters of Constantinople than by those of Rome or Florence: there was a direct and commercial communication with the city in which the emperors had taken up their residence; and if commerce ought to be considered as a vehicle of, or as influential on, the arts, we may easily conceive that all the portable works, which could be regarded as objects of speculation, must have had the character of those which were exported from the East. The Venetians, following the example of the Orientalists, studied brilliancy of colouring, and all the arts by which this effect could be obtained; and it would seem that not only did they profit by the rich colouring materials which commerce procured them, but long before the time of Giorgioni there were painters who studied the calculation of the masses of *chiaro-oscùro*, as well as the effects of opposite shades; so that this school cannot fail to be regarded as the parent of the modern colourists; since long before the Carpacci, the Basacti, and even the Bellini, they had always painted with vivid and durable colours.

lours *. Thus, I have no doubt that, by carefully pursuing these researches, we may be able to discover the source of the colouring in the most distant periods of this school. We may also add to these causes the custom of contemplating the highly coloured dresses of the Levantines who visited Venice; but their painters, not having the ancient models, nor the manners of the East, could not perpetuate the grand style which belongs to design.

Gothic School of the North.

Although the general appellation of Gothic has been given to the architecture of the middle age, the constant contemplation of ancient monuments must nevertheless force us to acknowledge certain distinctions in the various styles. The Italians, for example, gave the name of Arabo-tudesco to the style of the dome of the Great Church at Florence, built by Arnolfo in 1290; and they add that it is a mixture of the Moorish or good Greek with the Germano Gothic. In architecture a distinction has also been made among other styles, of the Saracen and the Saxo-Gothic. Sculpture has not been submitted to the same analyses, since its productions have been too much neglected; but the styles of painting were still more forgotten, and their different characters have not even been inquired into. The appellation of Gothic has therefore been given indiscriminately to all those paintings of a different physiognomy from those of the modern schools of Italy, and a great confusion of ideas has consequently been the result. Now, as the few paintings which were to be found in the North, before the existence of the Florentine school, were confined to some imitation of the *Greek-Christian* style, and presented but a very small number of images, it was thought that the Gothic style in painting was precisely that which had so abundantly filled France, Germany, and the whole of the North, with the vicious studies which were brought from Italy. We may therefore say that the school which has been called Gothic, originated much later than has been imagined, beginning only at the epoch when the influence of the ancient styles had become almost null; and when caprice, the barbarous taste of the times, and the

* All the movable paintings anterior to the innovation of John of Bruges, introduced at Venice by Antonello de Messina about the year 1450, were executed with white of eggs, wax, or gum; and when oil was introduced into the colours, it was merely used to finish, or to give permanence to, the pictures already far advanced by the first processes. Several Venetian pieces in the Napoleon Museum are painted in this way.

tone given by the manners of men, were the only guides which artists had: if architecture and occasionally sculpture, which was then tributary to it, preserved under the superintendence of priests and monks some of their essential qualities, painting also, which was more generally attainable, was almost entirely abandoned to the taste of those who attached themselves to it as a study. The painters of the North were no longer in contact with the ancient pupils of Rome and Constantinople; and notwithstanding the ingenuity of some figures in certain manuscripts, it can never be said that in these countries the art was upon the same level as in Italy. Some time afterwards the influence of the styles of some men very justly celebrated elsewhere, such as Albert Durer and Van Ecke, did nothing but degrade even the bad taste which prevailed; and it was after their time in particular, and after the pretended imitations of the Florentine school, that artists every where produced on glass, in altar-pieces, and in books, those works equally ridiculous as disgusting, and which are still to be met with daily: it is probable, therefore, that if in these countries there are occasionally paintings of some value, they have been the productions of foreigners invited there either by the princes and churchmen, or by some rich individuals. We may therefore repeat that, in the North, the style of almost all the paintings of the sixteenth century, notwithstanding certain qualities which sometimes render them valuable, is truly barbarous; and that this is the sole and true cause of that disgust which has been erroneously referred to causes originating in former ages, which were supposed to have been still more barbarous.

It will be no longer astonishing that it has been falsely supposed that painting took its rise in the sixteenth century, since, in fact, we generally find in the countries of the North styles which may be referred to the æra of the new schools, and since the churches, monasteries, and public buildings, were every where emblazoned with the mixed tastes of so many new painters; tastes which were even combined with that of Michael Angelo, the aspect of which gives to the public the false idea of works executed more than a hundred years before. This school was the æra of those attitudes and of those harsh and angular postures, the æra of the barbarous superfluity of back grounds and accessaries, perpetuated perhaps by the slight degradation of the colours of glass; and afterwards came the æra of those pantomimic and academical contorsions which were said

to

to have been brought from the Vatican: in a word, it was then that they introduced the strange and revolting custom of employing grotesque draperies of woollen stuff or moistened parchment: a style which even for gravest subjects too much resembles that which painters would study at present in our great cities, were they to frequent every public masquerade.

Let persons call these shameful perversions of painting Gothic if they please; they possess nothing in common with the fine arts of antiquity, and it is unfair to class them with the simple and rational productions of the middle age: certainly it was not these miserable daubings which Raphael made use of as his models; and it would not be absurd to suppose, that in these degraded times that contempt arose which the Italians have ever since cherished for ultramontane artists.

I conclude therefore from these observations, that Rome in the middle age produced paintings of a simple, rational, and regularly composed style, and that there are to be found in the works of that time subjects clearly conceived and finely expressed,—methodical compositions, and draperies of a happy and graceful flow: that the Greek school of the Lower Empire always presented figures of a severe and dignified character; that it still excels with the lustre of the colours of the East, and that it propagated this grand and ancient problem, *magnificence on simplicity*. I conclude that Tuscany witnessed the cultivation of painting by men of genius who formed for themselves a style animated but not very conformable to the elevation of the arts: that Venice exhibited in the most distant periods proofs of intelligence in colouring and *chiaro-oscuro*, and participated in some respects with the Greek taste of Constantinople: in a word, that the Goths of the North, who went in search of the arts to that Florence whose celebrity attracted the whole world, brought nothing from it, or from Rome, but superficial and altered ideas, or false and trivial traditions, which spread thoughout their own still barbarous country all those hideous images which I willingly abandon to the ill-nature of the malcontents.

There certainly were mixtures of these various manners in different countries; but the characters of these schools are not the less determined, and seem to be founded on the nature of things.

It has been already shown how erroneous the opinion of those has been, who, confounding all times and styles, do not admit of common sense as guiding the painters until the

the efforts of the celebrated men of the sixteenth century. Here we ought to enforce a principle which is very palpable, and easy of being retained, viz. that the art of painting is the purer, the nearer it approaches ancient times; that all which it has acquired in practical perfection, in subsequent ages, has only improved it in so far as artists have preserved a respect for ancient doctrines; and lastly, that if the manners and society of posterior times have restored its credit and activity, it is nevertheless true, that the best productions of the epoch called improperly enough the æra of the revival of letters, are those in which the new styles and imitations were not substituted for the ancient documents. The art therefore never perished; and when we compare the evils which it experienced at the periods of the conquests of the barbarians, with those which were brought upon it by the theories of the new students, we shall not hesitate to affirm, that it has suffered much more from the latter than from the former, and that their true destroyers have exerted their ravages much more directly and much more slowly than has been imagined.

Besides, in order to have a clear idea of these influences, and of the progress of the art, we must necessarily have seen and attentively considered the various productions on which these influences were exercised. But how much have these inquiries been despised! In fact, that person who, after having expressed his disgust at the sight of some of the vignettes of a manuscript of the sixteenth century, or of some badly stained glass much more modern, will inform us that the Gothic is a pitiful style; such a person, I say, who has seen those insufficient objects and some scraps of portraits, has never visited Tuscany, Venice, or Rome, has no knowledge of the fragments deposited in the voluminous collections of Bosio, Aringhi, Ciampini, Battari, and others: he passes by with disdain some valuable paintings which are frequently found in the cabinets of the curious, and he despises them because they are not decorated with the livery of our schools. In a word, the belief is too prevalent, that with the sixteenth century painting revived; and on the contrary the term *revival* is applied very improperly to the æra at which, perhaps, the art began to receive the last touches of degradation: and if eminent men and bold and original artists have adorned this memorable æra, if the too famous Michael Angelo by his pompous works has attracted the notice of all men; it is notwithstanding true, that he has stripped the ancient art of its *naïveté*, and the best pictures even of the present day

day are those in which we trace the beauty, the true simplicity, and the striking truths of nature. Thus the period of the corruption of the art was not when it lost its honours and consideration, but rather that it was no longer founded upon the grand principles which are its true supporters; and such is the immutable order of things, that all the splendour of the Cartoni, the Berninî, all the noise made by the Vanloos and the Bouchers never disguised the degraded state of paining.

Since therefore a new æra has commenced, and the art has risen by the force of genius alone, and without the aid of that cruel benefit of nature, which generally paves the way for the lustre of the arts by the previous darkness of destruction,—ought we not boldly to extinguish the prejudices which still pursue us, and reject with dignity all that is unworthy of our new glory?

But we shall now point out more precisely the various qualities observed in the last productions of the languishing and enfeebled art, and prove that they have been common at all times to the works of the most distinguished, both among ancient and modern artists.

[To be continued.]

XXVIII. *Remarks on Don* JOSEPH RODRIGUEZ'S *Animadversions on Part of the Trigonometrical Survey of England. By* OLINTHUS GREGORY, *LL.D. of the Royal Military Academy, Woolwich.*

To Mr. Tilloch.

DEAR SIR,—WHEN I say that I have been greatly surprised to see in the second part of the Philosophical Transactions for 1812, Don Rodriguez's animadversions upon part of the English Trigonometrical Survey, I conjecture that I am merely describing a feeling which has been more or less experienced by every man of science in this kingdom. The publication of an attempt by *a foreigner* to cast discredit upon a great national undertaking, in the Transactions of the most eminent philosophical institution of that nation, the Royal Society, that is, in a work which learned men on the continent contemplate as a fair picture of the science and genius of England, is, I believe, a thing unprecedented in the history of literature. If the great work which Don Rodriguez has taken upon himself to examine, had been really reprehensible, it would still have been extraordinary that he should be permitted to give his censures

censures currency in such a vehicle: but how much more extraordinary must it be thought, if on inquiry it shall appear that his strictures are causeless, and therefore unjust! This is an inquiry which every man of competent information, who has at heart the honour of his country, has a right to institute: and, however unpleasant the undertaking may in some respects be, I enter upon it without delay, because Colonel Mudge, whose reputation is so deeply implicated in this business, is at present prevented from giving Don Rodriguez's paper that decided and complete refutation which it will hereafter receive at his hands; and because his silence, though unavoidable, may be construed into defeat.

Impressed by these considerations, I propose in this communication to show, that the observations of this ingenious foreigner are, in all his main positions, unfounded; and although the matter under investigation is, in general, so nearly elementary, that any man of moderate scientific attainments might safely rest the truth of his assertions upon his own character and their intrinsic evidence; yet, lest it should be apprehended that, on this occasion, my judgement may be warped either by strong national feeling, or by private attachment, I shall fortify my positions, as I go along, by such authorities as neither Don Rodriguez nor any other person will be inclined to question.

Before I proceed to the points which Don Rodriguez selects as the basis of his animadversions, it may not be thought improper if I briefly advert to what appears his main, if not his sole object, in making those animadversions at all. I shall not, I hope, be deemed uncandid, if I say, that to me this object *appears* to be no other than the depression of English (and perhaps other) ingenuity and exertion, in order to the undue exaltation of the French scientific character. To this end, as it would seem, (for to what other purpose can it be?) we are told that in consequence of "the general impulse which the human mind received" from the French revolution, the members of their academy of sciences "invented new instruments, new methods, new formulæ," for the purpose of ascertaining the figure of the earth, &c. and commenced "an important undertaking almost the whole of which consisted of something new in science." I have no wish to depreciate the value of the discoveries and improvements of the French mathematicians; yet surely I may affirm that much had been done with respect to the grand topic in question, long before the French revolution. Did not Euler invent "new

methods and new formulæ" for this express purpose, and publish them so long back as the year 1753, in the Berlin Memoirs? Did not Dionis du Sejour much improve this branch of analytical theory? Did not Professor Playfair solve the general problem in all its *useful* varieties in the Edinburgh Transactions, before the publication of Delambre's investigations? Did not General Roy, and the subsequent English measurers, publish ingenious formulæ in the Philosophical Transactions; although Don Rodriguez insinuates that their methods are kept back? And, with respect to actual admeasurements, might not the Don have learnt from the Philosophical Transactions (see volumes lxxv. lxxvii. lxxx. &c.) that Government surveys were commenced in Scotland, so long back as 1745, by Lieut. Gen. Watson; that in 1775 the work was continued; that in 1783 an authorized committee or deputation of the mathematical philosophers of England and France met at Dover, to concert the best means of carrying a series of triangles from Greenwich to Paris; that the work was soon after pursued by the appointed persons in both countries; and that from that period it has almost regularly proceeded in England, whatever interruptions it may have experienced in France? How, then, can a writer insert in the Philosophical Transactions, where evidence to the contrary *abounds*, a paper from which, all who are unacquainted with the history of this important class of operations would conclude that they *originated* in the determination of the French to "establish a new system of weights and measures?"

To the same end apparently tends the Don's assertion that "the Swedish Academy of Sciences, *encouraged by the success of the operations conducted in France*, sent also three of its members into Lapland to verify *their* former measurement." For the natural tendency of this statement is to produce the belief, that the recent operations of the Swedish philosophers were in humble imitation of the French, and that they were undertaken for the purpose of verifying or of correcting *their own* former admeasurement; in both which respects the colouring given is widely different from the truth. The Lapland measure in 1736 was not conducted by Swedish, but by *French* academicians; and the correction of it was proposed long before the French revolution. The following are the true circumstances of the case, as I received them from a learned Swede. Melanderhielm, the venerable president of the Stockholm Academy, had almost from his youth doubted the accuracy

of

of the operations of 1736, and sought anxiously for an opportunity of repeating them; but waited many years before he could avail himself of a favourable conjuncture of circumstances; although latterly he had found in M. Svanberg, a *young* man of great talent and activity to conduct the operative part. After hearing of the new measure of a degree by MM. Delambre and Mechain, he wrote to some of the French mathematicians on the subject, but with no intention of soliciting them to visit Lapland. Soon after this, Bonaparte, at the suggestion of the then National Institute, *wrote* a letter personally to the late king of Sweden, requesting permission for some members of that body to proceed to Lapland, in order to determine an arc of the meridian. That high-spirited young monarch replied, that he would consult his own Academy of Sciences at Stockholm, whether such an operation was desirable for the interests of science; and, if they were of that opinion, he had no doubt he could find *Swedish* mathematicians competent to the undertaking. Hence MM. Svanberg, Ofverbom, Holmquist, and Palander were appointed to examine and repeat the measure of the French academicians; and this is what Don Rodriguez terms the expedition of *three* of the Swedish academicians "to Lapland to verify *their* former measurement!"

With the same spirit it is natural to suspect Don Rodriquez speaks of Colonel Mudge as "*a skilful observer*," and merely such, adding that "one cannot but admire the beauty and perfection of the *instruments* employed" by him: while, when he characterizes the labours of the French measurers, he assures us they "merit the *highest* degree of confidence;" and, "by the sanction of such an union of talents, give such a degree of credit and authenticity to *their* conclusions, as could scarcely be acquired by other means." I shall not animadvert upon this invidious contrast; but simply remark here, that the Don adopts a strange method of verifying his positions. He admits that Colonel Mudge is a *skilful* observer, who knows very well how to employ his instruments; and, that there may remain no doubt on that head, publishes a long paper to prove, or at least to show it probable, that he has made a *mistake* of $4\frac{1}{2}$ seconds in the determination of a zenith-distance. This animadverter has, as he assures us, gone through *all* the Colonel's computations by different processes, and found them correct, or only evincing very trifling discrepancies, such as may naturally arise from the diversity of methods: yet he cannot find in his heart to drop a single word of

commendation on him as a computer, or as an investigator.

The preceding remarks will suffice, I apprehend, to render manifest the probable object of Don Rodriguez's paper. I shall now proceed to inquire how far the reasons assigned by this gentleman bear him out in his attempt to throw suspicion upon the operations of Colonel Mudge in measuring an arc of the meridian. The Don's paper, it is true, is rather desultory and unconnected; but I trust I shall neither misrepresent him, nor do injustice to his arguments, by endeavouring to reduce them to the following order.

1. Colonel Mudge's observations must be wrong somewhere, because his results do not correspond with those of the French measurers. This is not positively affirmed, but every where strongly implied: for Don R. assumes his value of the radius of the earth's equator from the French measurements and computations; and he takes it for granted, that the fraction exhibiting the ratio of the difference of the earth's axes to the major axis, technically termed the *compression*, lies somewhere between those limits ($\frac{1}{330}$ and $\frac{1}{310}$) which a superficial observer would adopt as most suitable to the French operations. Such assumptions, by the way, are neither consistent with fair criticism nor with sound logic: for the grand object in measuring arcs of meridians is to *determine the ratio* of the earth's axes; and when in the course of any such admeasurements avowedly remarkable anomalies arise, it is a mere *petitio principii* to conclude that there *must* be some error in the astronomical *observations*, because irregularities as great or greater than those which the operations indicated result from computations resting upon a gratuitously assumed ratio.

But some of the French operations at home, compared with those at Peru, give about $\frac{1}{309}$ for the compression *. Be it so. That is no reason why any such ratio should be adopted as the test by which to try the accuracy of English *observations*. Don Rodriguez himself, when applying the same test to the French meridian, thereby detects irregularities, and great ones too; yet does not whisper the gentlest hint that they were occasioned by inaccurate observations. Why not? Because M. Mechain "handled instruments with *great* delicacy, and was possessed of *peculiar* talents for this species of observation." So that a gratuitous assumption should suffice to render English observations doubtful, while it leaves the accuracy of French

* *Biot, Astronomie Physique*, tom. i. p. 159.

ones

ones unimpeached. To me it appears that a candid critic would in analogous circumstances make analogous inferences; and not sift one class of results to the bottom, while he satisfies himself with merely glancing at the surface of the other class. Had he examined the French measures a little more minutely, he would, instead of adopting them as his standard, have found that they exhibit far too great irregularities to be entitled to that honour. Taking the results of the operations of Delambre and Mechain, as subdivided naturally by the assumed stations at Dunkirk, the Pantheon at Paris, Evaux, Carcassone, and Montjouy, and applying to them the principle developed by Legendre, in which "the sum of the *squares* of the errors is made a *minimum*," the requisite compression is $\frac{1}{148}$; and even then, the deviations from what the theory would require are, at Dunkirk $-2\cdot''23$, that is, nearly $2\frac{1}{4}$ decimal seconds; at the Pantheon, $+5\cdot''63$; at Evaux, $-4\cdot''79$; and at Carcassone, $+1\cdot''34$. Here the compression which agrees best with the observations is more than *double* what it ought to be. If a medium compression had been chosen, the errors at the several stations would have deviated still further from the probable errors of observation. Don Rodriguez will find this confirmed by Puissant, *Géodésie*, p. 137, 141, and by Laplace, *Exposition du Systême du Monde*, liv. i. ch. 12. After he has duly reflected upon the deductions of those philosophers, he will perhaps be convinced that he has been rather precipitate in taking the French operations as a standard.

But 2dly, This writer infers that there must be some error in Col. Mudge's observations, because they tend to show that the terrestrial spheroid is very irregular. All the measurements "which have been hitherto made in the northern hemisphere are (he tells us) *extremely satisfactory by their agreement*, and give us great reason to presume that the general level of the earth's surface is elliptical, and *very regularly so*." "There would not have remained the *smallest doubt* respecting the earth being flattened at the poles," but for the "measurement performed in England." But "this measure alone would lead to the supposition, that the earth, instead of being flattened at the poles, is, in fact, more elevated at that part (the author means *those parts*) than at the equator, or, at least, that its surface is *not that of a regular solid*." The degrees, in fact, increase as the latitudes diminish; which, says Don Rodriguez, "excites a suspicion of some incorrectness in the observations themselves;" whereas, the only fair inference is, that

that an *insular* situation is very ill fitted to promote the determination of the figure of the earth.

Let us see, however, how "*satisfactory*" former measures have been "by their *agreement*," and how completely they prove that the earth's surface is "*very regularly*" elliptical. Lacaille's degree in lat. 45° N. compared with Bouguer's at the equator, gives for the compression $\frac{1}{288}$. The degree in Maryland, with Bouguer's equatorial, gives $\frac{1}{300}$. The Spanish degree at the equator, with the French degree lat. 45°, gives $\frac{1}{303}$. Boscovich's Italian degree, lat. 43°, compared with Bouguer's at the equator, gives $\frac{1}{363}$. Bishop Horsley, by a geometrical mean of twelve different ellipticities, obtains $\frac{1}{200.4}$. Boscovich, taking a mean from all the measures of degrees, so as to make the positive and negative errors equal, obtains $\frac{1}{248}$. Lalande, by comparing Father Leisganig's degrees in Germany with eight others in different latitudes, gets $\frac{1}{311}$. And the recent measures in France give, as we have seen, $\frac{1}{148}$. Such is a summary of the evidence from which it is to be concluded that the earth is "elliptical," even "very regularly so." General Roy, who had got a habit, not very uncommon among scientific Englishmen, of deducing reasonable conclusions from anomalous appearances, and not twisting them to suit a fanciful hypothesis, assumed *seven* different spheroids of varying ratios between $\frac{1}{179}$ and $\frac{1}{540}$; and, on finding that none of them corresponded so uniformly as might be wished, with the operations in different latitudes, made these inferences: "Hence it is obvious, that the arcs of an ellipsoid, however great or small the degree of its oblateness may be, will not *any way correspond* with the measured portions of the surface of the earth." "Hence it is that philosophers are not yet agreed in opinion with regard to the figure of the earth; some contending, that it has *no regular figure*, that is, not such as would be generated by the revolution of a curve around its axis." And again, after specifying some other facts, "From all which we may conclude, that the earth *is not an ellipsoid.*"

Nor is this opinion peculiar to Gen. Roy: it is common, I believe, to all who have contemplated the subject, except Don Rodriguez. Thus, Puissant, at p. 187 of his *Géodésie*, says, "La comparaison des divers degrés mesurés à l'équateur, en France, en Pensylvanie, etc. donne lieu à décider que les méridiens sont différens entr'eux, et n'ont pas la forme elliptique." And at p. 222, "D'où l'on doit conclure que la terre *n'a point* la forme régulière que l'on serait tenté de lui attribuer." To the same purpose writes

Laplace,

Laplace, at p. 56 of his "*Exposition:*" "Les degrés du nord et de France donnent $\frac{1}{146}$ pour l'ellipticité de la terre, que les degrés de France et de l'équateur donnent égale à $\frac{1}{334}$: il paroit donc que la terre est sensiblement différente d'un ellipsoide. Il y a même lieu de croire *qu'elle n'est pas un solide de révolution*, et que ses deux hémisphères ne sont pas semblables de chaque côté de l'équateur."

It is curious, however, to observe that, notwithstanding this extreme want of uniformity, in the results furnished by terrestrial admeasurements, those which are deduced from astronomical theory, and the oscillations of pendulums, correspond very nearly. Thus, Laplace's deduction of the compression from the lengths of pendulums in different latitudes, is $\frac{1}{335.78}$. (See Puissant, *Topographie*, &c. p. 66.) Clairaut's well known modification of Newton's theorem, derived from the diminution of gravity, gives $\frac{1}{304}$. The phænomena of the precession of the equinoxes and the nutation of the earth's axes give $\frac{1}{304}$ for the maximum limit. A lunar inequality in longitude depending upon the earth's ellipticity, and expresssed by $-20''.987 \sin ☊$ of the moon in longitude, requires the compression to be between $\frac{1}{334}$ and $\frac{1}{305.05}$, but nearest the latter limit. And a lunar inequality in latitude, depending also on the compression, and expressed by $-24''.6914 \sin ☽$, requires the compression to be between $\frac{1}{334}$ and $\frac{1}{304.6}$, still leaning to the latter limit. So that the ratio of the earth's axes, as deducible from these independent theoretical considerations, lies within much narrower limits than we can get in any other way. But this does not affect the truth of the preceding remarks. It serves principally to show that, whatever may have been the derangements of the terrestrial spheroid since its original formation, they are not such as have differently affected the several phænomena occasioned by its aggregate attraction: while a very slight consideration of the effects of the deluge, of earthquakes, of volcanic operations, of extensive dislocations of strata, &c. may serve to convince us, that however regular the earth might once have been in its general shape, there is now no reason to expect that "very regular" surface from which Don Rodriguez persuades himself there ought to be no essential deviation.

3. Don Rodriguez is further confirmed in his opinion that there must be an error in the observations, especially at Arbury Hill, of "nearly five seconds," because he thinks no such anomaly as that can fairly be ascribed to the effect of local attractions. He does not deny "that irregularities of

of the earth and local attractions may occasion considerable discrepancies ;" yet he does not believe they can ever produce a deviation of the magnitude just specified. Here again he is at war with the decisions, I believe, of all preceding philosophers who have directed their attention to this subject. There are, obviously, three causes which may jointly or separately occasion a deflection of the plumb-line from the true perpendicular to the earth's surface; namely, an insular situation, the attraction of mountains, and strata of unequal density beneath the surface: and either of these may be productive of considerable effects.

To arrive in the easiest manner at an estimate of the effect upon a plumb-line arising from observations made in an insular situation, let Don Rodriguez imagine the simple case of a triangular island so posited on the surface of an aqueous spheroid, that a meridian shall run along from its vertex, directed northward, to the middle of its base; he will perceive that, in such a case, as an observer proceeded from the south towards the north, there would be a constant variation in the deflection of the plumb-line, in such manner that there would be only one point on the meridian, where the attractions occasioned by the island itself should be so counterpoised and adjusted, that the true and observed vertical lines should correspond. Pursuing this hypothesis, with the requisite modifications, for a neighbouriug continent on the south and an immense ocean north, he will find that the singular order exhibited by the English estimates of degrees, though an unexpected, is by no means an unnatural, consequence of our insular situation. Dr. Hutton has treated this very point with his usual perspicuity, in a valuable note at page 198, vol. ii. *New Abridgement of the Philosophical Transactions*, published in 1803. That note is too long to be copied into this place; I shall therefore merely transcribe the Doctor's concluding inference: "Hence also it follows that insular situations must be worst of any, having the plumb-line deviating to the north at the south end of the line, to the south at the north end, to the east at the west side, and to the west at the east side; thus producing errors in all observed latitudes and longitudes."

Laplace most probably alludes to this kind of effect, at p. 59, "*Exposition*," where he speaks of the *much more extensive attractions than those of mountains*, of which the effect is sensible in Italy, *England*, &c.

That the deflections of the plumb-line, and the consequent estimate of the lengths of degrees, must be greatly affected

affected by hills and valleys, is also very manifest. Professor Playfair, after describing the irregularities thus occasioned in the degree at Turin, adds, "There are, *no doubt*, situations in which the measurement of a small arc might, from a similar cause, give the radius of curvature of the meridian *infinite, or even negative.*" See Edinburgh Transactions, vol. v. p. 5. And Dr. Maskelyne, after treating of Mason and Dixon's degree in North America, says, "Mr. Henry Cavendish having investigated several rules for finding the attraction of the inequalities of the earth, has, upon probable suppositions of the distance and height of the Allegany mountains from the degree measured, and the depth and declivity of the Atlantic Ocean, computed what alteration might be so produced in the length of the degree; and finds that it may have been *diminished by* 60 *or* 100 *toises* by these causes. He has also found, by similar calculations, that the degrees measured in Italy, and at the Cape of Good Hope, may be *very sensibly* affected by the attraction of hills, and defect of the attraction in the Mediterranean Sea and Indian Ocean." Phil. Trans. vol. lviii., or New Abridgement, vol. xii. p. 578.

With respect to the third cause of irregularity Puissant, *Géodésie*, p. 137, remarks that "anomalies in the latitudes are *doubtless* produced by local attractions which change the direction of the apparent vertical." And Professor Playfair, in the excellent memoir I have just quoted, (a memoir, it should be recollected, which was written *five* years before the remarkable anomalies in the English measures were known,) affirms that "from suppositions no way improbable, concerning the density and extent of masses of varying strata beneath the surface, he has found, that the errors thus produced *may easily amount to ten or twelve seconds.*" "This cause of error (as he justly remarks) is formidable, not only because it may go to a great extent, but *because there is not any visible mark by which its existence may always be distinguished.*"

Here, then, are *three* sources of deflection from the true plumb-line, neither of which is correctly appreciable in all circumstances, yet of which each may be not only perceptible but important; and the concurrent effect of all may, doubtless, be very considerable. Yet Don Rodriguez is unwilling to attribute a deviation of four or five seconds to any or all of these causes.

4. This writer infers that mistakes must have occurred in the observations, because the sum of other "errors will

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be found in the estimate of the entire arch, and will increase in proportion to the extent of the arch measured: but in the English measurement we find exactly the reverse of this." Here he assumes the principle proposed by Boscovich, but condemned by Laplace, for a reason thus briefly assigned by Puissant:—"La solution donnée d'abord par Boscovich est *vicieuse* en ce qu'elle est fondée sur une *hypothèse inadmissible*, savoir, que les erreurs dans le mesure des arcs du méridien sont proportionelles à leurs longueurs."

5. He concludes that there must be "an error of some seconds in the observations of the fixed stars," because "the results of the observations made on different stars differ no less than four seconds from each other." Now, what are the facts on which this inference rests? Simply these: that the only two stars which indicate any such difference, in the whole series of observations, are μ Draconis, and ζ Ursæ; that they give a difference of $4''.19$, not in the amplitude of the arc between *Dunnose and Arbury Hill*, but of that between Dunnose and *Clifton*; and that whether those two stars be rejected, or retained with the other *fifteen* employed in finding that amplitude, they will not occasion a difference of a quarter of a second in the result. How, then, can a fair investigator bring this as a reason for an alleged inaccuracy, when it obviously cannot apply to the case? And what must be thought of his impartiality, if it shall appear that *even in this respect* the observations of the French and of Major Lambton, which he so manifestly prefers to the English observations, are far more open to censure? Allow me, therefore, just to make the comparison.

Of the English observations *none* are suppressed, (the observers going upon the principle explained by Simpson in his "*Tracts*," which clearly establishes the propriety, if not the necessity, of taking the mean of a *number* of observations,) and yet no irregularity of consequence, except the one above specified, appears. But, it may be seen from p. 72, *Discours Préliminaire*, tome i. *Base du Systême Métrique Décimal*, that no less then *sixty-eight* of the French observations upon β Ursæ majoris were rejected, and termed *bad*; for no other reason that I can perceive, than that if they had been employed they would have given the latitude of Dunkirk about a second less than the observations of the pole-star gave it. Let Don Rodriguez reflect upon this, and then repeat that the French operations "*merit the highest degree of confidence.*" But this is not all.

all. From p. 39 of the same *Discours Préliminaire,* it appears that *three* stars only were *selected* by Mechain at Montjouy, in consequence of the coincidence of the results arising from them. Among the stars rejected was ζ *Ursæ,* because different observations gave a difference of 4″. So that the French also detected an irregularity respecting this star. They assign, however, a wrong reason for the fact: for they attribute it to errors in Bradley's table of refractions; while the truth is, that ζ Ursæ is a *double* star, by no means easy to observe properly. Indeed it appears not only from the observations of Col. Mudge, &c. but from those of Dr. Herschel, (Phil. Trans. vol. lxxii. New Abridgement, vol. xv.,) that both μ Draconis and ζ Ursæ are double stars; that, of the former, the two constituent stars appear equal, both white, and not easily distinguishable, and at the distance of 4·″35 from each other, mean measure; and that, of the latter, the two are considerably unequal, and the largest difficult to bisect. Hence, Herschel's observations completely confirm those of our trigonometrical surveyors. See also the Catalogues of Wollaston and Bode.

Let us next inquire how far Major Lambton's observations, which Don Rodriguez also seems to delight in eulogizing, deserve to be preferred to Col. Mudge's. From p. 356, vol. x. Asiatic Researches, we learn that the Major's observations upon α Serpentis were 14, of which two were 5° 57′ 3·″38 and 5° 56′ 53·″98, furnishing a difference of 9·″3; more than *double* the difference that has been found in the English observations of which the Don complains! At p. 357 again, we have a register of sixteen observations upon ν Aquilæ, of which two differ by 6·″77. At p. 358, we have eighteen observations upon Atair, of which two differ by 5·″38. There are also some other palpable differences in Major Lambton's results, as deduced from *different* stars. The greatest is between Atair and Markab; being 5·″48. Atair, from the number and agreement of its observations among themselves, should be correct in zenith distance; yet it gives the latitude of the station, Dodagoontah, *less* by 3·″4 than the mean of the nine stars employed by Major Lambton exhibits it: and the latitude found from a mean of the four northern stars is 2·″04 *greater* than the latitude found from a mean of the five southern stars. Discrepancies of more than 4″ may likewise be frequently found in the observations recorded in vol. viii. of the "Researches." Most of them are probably

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in great measure attributable to the imperfections in Major Lambton's sector, which is only of five feet radius (while the English is of eight feet), and is provided with but few comparatively of the requisite means of adjustment: but whether they are to be ascribed to the observer or his instruments, they prove that Don Rodriguez has been rather precipitate in saying, "the same Major Lambton, who has *succeeded so well* in Asia, and is in possession of such *perfect instruments* for the purpose, would be *singularly qualified* for a similar undertaking in Africa." In matters which admit of examination and proof, it is not the custom with Englishmen to bow at once to the authority of a mere *ipse dixit*. Was Don Rodriguez really ignorant that, with respect to accuracy of observation, the English proceedings are thus greatly superior to those of the French and of Major Lambton! If so, how greatly is he to be pitied for writing so much on a subject he had previously so little considered! If he *was* aware of this superiority, how much more is he to be pitied, for giving so unfair and unnatural a representation of the business before him!

From one or other of the reasons I have thus examined, Don Rodriguez says, "it is *almost beyond a doubt* that it is to errors in the *observations* of latitude," the singularity in Col. Mudge's results must be ascribed. There *must* be an error of some seconds in the observations, "especially at Arbury Hill." And he asks "How is this to be discovered?" How? Why, by simply *repeating* the observations at Arbury Hill. The position of the station is so clearly described in the Philosophical Transactions, that any person may find it within twenty feet; and the farmer who owns the field can show the identical spot. Don Rodriguez or some one of his friends has doubtless *handy* circular instruments of the French construction, by which the zenith distances could readily have been taken, and then the correctness or incorrectness of the English observers might have been proved in a way from which there could be no appeal. Though to be sure, if that plan *had* been adopted, and the English results had in consequence been verified, Don Rodriguez's paper could never have appeared.

There is, however, a method of determining the point, even without taking this trouble. Having then shown, I trust satisfactorily, that Don Rodriguez's reasons for imputing an error of four or five seconds to the English observations are nugatory; I shall now proceed, with all possible conciseness, to show that *there cannot be an error of*

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one second either in the observations at Arbury Hill, or at Dunnose; and those at Clifton are, by the Don's own concessions, out of the qnestion.

First, the manner of *fixing* the zenith-sector could not lead to error. For, "to procure for the external stand (says Col. Mudge, Phil. Trans. 1803), and thence for the whole apparatus, a firm foundation, I caused four long stakes to be driven into the ground, (one for each foot of the stand,) to which its feet were firmly screwed down. The surfaces of the stakes were cut off smooth, and brought into the same horizontal plane, by which means the interior frame and sector were placed much within the limits of their several adjustments." The whole was inclosed in a suitable observatory.

Don Rodriguez may perhaps think the French method of fixing their instruments, on some occasions, preferable to this. The reader shall judge. Their instruments, both for taking horizontal and vertical angles, were sometimes placed on *tottering* stages, so as to give anomalies in the angles of from 5″ to 8″; furnishing, as Delambre terms them, "le tourment des observateurs." Thus, at p. 46, *Discours Préliminaire*, we are told that at Chatillon there was a high wooden stage erected for an observatory, in which the carpenter had so badly done his work that "le *moindre* vent agitait toute la machine, de manière, non seulement à rendre les observations moins sûres, mais *à inquiéter les observateurs*." And on turning to p. 174, tome i. it will be seen that the observers had not to contend with a gentle gale; for they there tell us of the "*grand vent* qui agitoit le signal et l'instrument." The whole was *blown down* shortly after. Will Don Rodriguez place reliance on observations made from such a platform in such a wind, and notwithstanding doubt the observations made with a stable instrument by the English? And let him not forget, that whatever error was thus occasioned in the distance between Boiscommun and Chatillon, is more than doubled in all the remaining triangles of the series, by reason of the bad shape of the triangle, Chatillon, Boiscommun, Chateauneuf.

If no error in the English observations can be fairly imputed to the manner of *fixing* the zenith sector, neither can any be ascribed to the "*construction*" of the instrument itself. This was most positively declared by two very excellent judges, the late *Astronomer Royal*, and the Hon. *Henry Cavendish*, on their close examination of the instrument. It will also be inferred, without hesitation, by all competent

competent judges, on reading the description of it in the Phil. Trans. for 1803. To those who have seen neither the instrument nor the description, it may suffice if I remark, that the equality of the divisions on the arch is evinced from this consideration, that on running the micrometer screw from division to division over the whole arch, there was nowhere an indication of an error amounting to half a second; and that the instrument still continues free from important "derangement," is tolerably well proved by this, that the line of collimation has been *constant* during all the observations and all the journeyings of the sector, and that it still continues the same.

In the next place, it may be remarked that no error in observation can be imputed to a deviation from "*vertical* position" in the sector. Important inaccuracy in this respect is precluded by the great length of the axis, by which the instrument is rectified; and by the ready and certain means of placing the plumb-line directly over the illuminated *dot* which marks the middle of the axis, or true centre of the divided arch. For want of this admirable mode of correction, all previous instruments are necessarily imperfect. It appears from Phil. Trans. for 1803, pp. 405, 406, that when the instrument is adjusted in one position by means of the plumb-line and dot, it is turned to a position at right angles to the former, and the adjustment confirmed; and this being the case in these two situations, the instrument must necessarily be vertical in all others.

Various reasons may be assigned to show that the sector could not, at any of the stations, be out of the plane of the *meridian*. I shall select only two or three. As 1st: If the sector were inclined to that plane, just so much would the path of any star in its apparent motion be inclined to the horizontal wire of the telescope; instead of which, both Col Mudge and Capt. Colby assure me, that when a star came into contact with the wire, the light of the star would appear on both sides of the wire for about three-quarters of a minute of time, the light on each side being equal at the central wire: which of itself is a positive proof. But, 2dly: Had the sector been out of the plane of the meridian, the times of the transits of the extreme stars employed, as compared with two excellent time-keepers, must have shown it. Further, the errors arising from a wrong plane of the meridian being comparatively very great in the extreme stars and small in those near the zenith, it would follow that the error in Capella, which is almost at the extremity of the arch, would be great, compared with those in

β Draconis,

β Draconis, κ Cygni, &c. which were within a small distance of the zenith. But the amplitude of the arch between Dunnose and Arbury Hill, as derived from Capella, is 1° 36′ 20·″02, while those derived from the other two stars are 1° 36′ 19·″42 and 1° 36′ 19·″94: a coincidence which proves that the instrument could not possibly have any perceptible deviation from the plane of the meridian at either station. Other reasons for coming to the same conclusion will appear, on attending to the precautions in adjusting by double azimuths, &c. as described in the Philosophical Transactions.

The correct position of the sector in *all* respects is further proved from this: that the observations, however distant in point of time when the proper corrections for aberration, nutation, &c. are applied to them, reduce always very nearly to the same mean place.

Hence it must be obvious that no error could arise, as Don Rodriguez suspects, from the *instrument*, whether in "vertical position, construction, or some accidental derangement." I shall now advance still further, and prove *that there is no error, in fact*. For, if there were any error in the zenith distances at Arbury Hill, it would at once be detected on comparison with the observations at Blenheim. Now the distance between the parallels of latitude of Blenheim and that hill, 139,822 feet, furnished by the survey, gives for the corresponding celestial arch 22′ 59·″33, while the observations of γ Draconis at Blenheim, compared with the observations upon the same star at Arbury Hill, give 22′ 59·″6. So that there *cannot possibly* be an error of half a second at Arbury Hill, unless the observations for five successive years at Blenheim were all wrong: and Blenheim observatory, be it recollected, has been long celebrated for the excellency of its instrument, and selected even by *Svanberg* for the accuracy of the observations there made. So again, with regard to the Dunnose station, the latitude of Portsmouth observatory, as inferred from the said station and the data in the Trigonometrical Survey, is 50° 48′ 2·″65; while the Requisite Tables, the edition of 1781, give it 50° 48′ 3″. So that the observations at Dunnose *cannot possibly* err half a second, unless there was an error made by Witchell and Bayly in determining the latitude of Portsmouth observatory, with an admirable mural quadrant by Bird. These two deductions, then, completely exclude sensible error at Dunnose and Arbury Hill: and these inferences, it is evi-

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dent, might as easily have been made by Don Rodriguez as by me.

This gentleman may find still further confirmation of the truth of the whole survey, if he will examine the operations by which the meridian of Dunnose is extended to Burleigh Moor, and those for carrying on a new meridian from Black Down to Delamere Forest. These, it is true, are not to be found (for what reason I cannot say) in the Philosophical Transactions. But they may be seen in the third volume of the Trigonometrical Survey, published in 1813 by order of the Board of Ordnance; a volume with which some of Don Rodriguez's friends in England are doubtless acquainted.

As a last corroboration of the whole portion from Dunnose to Clifton, amounting to 2° 50′ 23·″38; let me add that, when compared with the meridional arch of 3° 7′ 1″ at Peru, by means of the valuable theorem investigated by Professor Playfair (Edinburgh Transactions, vol. v. pp. 8, 9.) for the comparison of *large arcs*, it produces $\frac{1}{332 \cdot 68}$ for the resulting *compression*. While Svanberg (p. 192, *Exposition*) gives $\frac{1}{331 \cdot 443}$ for the compression, as deducible from a comparison of *his* measure with that at Peru.

Thus, we have confirmation upon confirmation, of the correctness of Col. Mudge's operations, both general and particular; and of the extreme rashness with which Don Rodriguez has affirmed that "it is *very evident* that the zenith distances of stars taken at Arbury Hill are affected by *some considerable error*." The matter in question might, as you will perceive, have been settled in narrower compass: but the celebrity of the Institution under whose auspices the Don's animadversions are circulated, seemed in some measure to call for a tolerably full reply to his paper.

For the reply here presented the public must consider me alone as responsible: and I trust that when the two papers have been compared, I shall not be thought to speak incompatibly with the courtesy due to a foreigner, or the respect due to a brother mathematician, when I say that Don Rodriguez has *completely failed* to establish the point, respecting which he ought to have felt *certain* before he commenced his strictures.

Royal Military Academy,
Woolwich, March 5, 1813.

OLINTHUS GREGORY.

XXIX. *De-*

XXIX. *Description of a hanging Scaffold to be used in repairing or painting Outside Walls of Houses. By Mr.* Joseph Davis*.

Sir,—Having to repair and beautify the front of my house, which is called the Minor-Theatre, in Catharine-street, Strand, I invented a machine which answers for such purposes much better than a scaffold, and saves considerable expense. As I conceive this contrivance may be beneficial to the public, I should be happy to submit a model thereof to the Society of Arts, &c. for their inspection.

My machine is twenty-six feet long, and cost me two pounds ten shillings, and when no longer wanted for this purpose, the timber is worth two pounds. A single machine may be made thirty-six feet long, and united to any length, and when not in use, may be folded up and put by as a common ladder.

On this plan of a scaffold, there is no occasion to break up the pavement, or to give the least interruption to passengers in the street.

I am, sir,

Your most humble servant,

Catharine-street, April 14, 1812. Joseph Davis.

To C. Taylor, M.D. Sec.

Reference to the Engraving of Mr. Joseph Davis's *temporary Scaffold for repairing the Outsides of Houses.* Plate VI. fig. 1.

This simple and effective contrivance consists of nothing more than a couple of planks A, to which two others BB are nailed, forming a sort of trough or moveable scaffold, on which the workmen stand; which is suspended at any height at pleasure. CC, DD, are two frames of wood, in which the trough or scaffold is fixed; in the top cross-pieces of these frames, two pulleys, E and F, are fitted, and round these the ropes by which the scaffold is suspended are passed; the ends *a a* of these ropes are made fast to two beams, or scaffold poles, G and H, which project out of the upper windows: or they may be fixed over the parapet, or by any other means, as is thought proper; two single pulley blocks, *g g*, are also suspended from these poles,

* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, for 1812.—The silver medal of the Society was voted to Mr. Davis for this communication, and a model of the apparatus is preserved in the Society's Repository.

poles, and the rope, after passing under the pulleys E and F, passes over the pulleys, in these two blocks, and the ropes or falls, *h* and *i*, come down to the machine, and are made fast to any convenient part of it: therefore, by drawing these ropes, the workmen can, with the greatest ease, raise or depress the suspended scaffold to any place where it is wanted for work.

XXX. *Method of relieving a Horse from a Cart when fallen down in its Shafts.* By Mr. JOSEPH MARTIN *.

SIR,—I BEG you will do me the honour to inform the Society of Arts, &c. that I have just now completed an invention to relieve horses when they fall down in the shafts of a heavy loaded cart or carriage, and I will wait upon the Society with a model for inspection whenever they will please to appoint.

Believe me to be, sir,

Your most obedient servant,

176, Fleet-Street, Feb. 19, 1812. JOSEPH MARTIN.

To C. Taylor, M.D. Sec.

Reference to the Engraving of Mr. JOSEPH MARTIN'S *Method of relieving a Horse when fallen down in the Shafts of a loaded Cart.* Plate VI. fig. 2.

Figure 2. of Plate VI. represents a cart, in which the horse having fallen, has been relieved by detaching the shafts from the cart, which is provided with temporary legs or stays, DD, to support the weight of the front part, and prevent its falling any lower until other means can be resorted to for raising it again.

The figure represents a common cart. AA represent the shafts detached and lying on the ground; the connection is formed with the cart in use by two screw-bolts, one of which is seen at *b*, passing through the bed of the cart, and also through the iron hinges *aa*, by which the shafts are united to the cart: besides these screw-bolts two steady pins, or bolts without heads, also pass through holes in the shank of each of the hinges, and two nuts BB are screwed on to fasten them when the cart is in use; but if the horse falls down, so that the weight of the cart comes to rest

* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce,* for 1812.——The Society voted ten guineas to Mr. Martin for this communication, and a model of the contrivance is preserved in the Society's Repository.

upon

upon the fore-ends of the shafts, he is confined between them by the weight, and prevented from rising. Mr. Martin's method of relieving a horse in such a situation is thus effected. The locking bar E is first removed from the staples *e e* of the shafts, with which the body of this, like most other carts, is provided, for the purpose of turning up and discharging its contents: this being done, a man must creep under the cart from behind, and first put down the legs or stays DD, which were before turned up out of the way beneath the cart; then unscrewing the nuts BB, the shafts fall down, completely detached from the body of the cart, as represented in the figures; and nothing then prevents the horse from getting up, the cart remaining supported in front by the legs DD. C is an additional leg folded up beneath the cart; it is of such a length that it will, when set upright, support the cart in an horizontal position, and is used to support the cart when lifted up whilst the shafts are fixed on again: the legs DD are provided with iron hooks, one of which is seen at F, which retain them steadily in their places, when the weight of the cart tends to throw them forwards: the leg C is provided with a similar hook.

XXXI. *An Attempt to determine the definite and simple Proportions, in which the constituent Parts of unorganic Substances are united with each other.* By JACOB BERZELIUS, *Professor of Medicine and Pharmacy, and M.R.A. Stockholm.*

[Continued from page 90.]

IV. COPPER AND SULPHUR.

1.) TEN grammes of the pure copper, called copper ashes, or dust, were well mixed with ten grammes of pure sulphur, and strongly ignited in a glass retort, furnished with a receiver and a tube of safety; they acquired an addition of 2·56 grammes. This copper dust is an extremely fine powder obtained in the smelting-houses when the metal is refined. [It consists of minute grains, which are thrown up to a height of six or eight inches, while the copper is cooling, after the removal of the ashes and cinders from the furnace, and which generally fall down again in the form of a fine metallic shower, except when they are intercepted by a shovel, which is moved backwards and forwards for the purpose.—Gilbert.]

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2.) In a repetition of this experiment, the increase of the weight of the copper amounted to 2·6 grammes.

Several other experiments exhibited a still greater increase of weight; but I do not particularly describe them, since the results are always somewhat too great, and do not perfectly agree, a circumstance probably depending on an oxidation, which the superfluous sulphur is incapable of reducing.

The following experiment however deserves to be noticed. I had put thin laminated copper in a small retort with sulphur, some of the plates projecting $1\frac{1}{2}$ inch beyond the sulphur. When the temperature was raised until the copper began to unite with the sulphur, the mass became heated, but was not ignited, because the sulphur was in excess, and the projecting parts of the plates did not enter into combination with the sulphur. When I continued to increase the heat, the small retort became completely full of sulphurous gas, and before the mass at the bottom of the retort was ignited, the plates were inflamed, and burnt with a very bright light, exactly as during combustion in oxygen. The copper therefore condensed the gaseous sulphur with the appearance of fire. Since copper combines also with solid sulphur with a similar appearance of fire, I was desirous to know if the phænomena could be derived in this case also from condensation. I weighed therefore the sulphuret of copper, which I had obtained, in water: its specific gravity was 4·76, that of the laminated copper 8·723, and that of the sulphur 1·99. Now four parts of copper had absorbed one of sulphur; so that the mechanical mixture of four parts copper and one sulphur is denser than the chemical compound, in the proportion of one to ·9124. The sulphuret had therefore expanded, and almost in the same proportion as the laminated copper would have done by fusion: consequently the change of bulk could not be the cause of the appearance of fire. Whence then were the matters of light and heat in this case derived? The case is evidently similar to that of the combustion of carbon in oxygen, where the heat is intense, although the carbon is expanded. When a piece of charcoal is ignited in nitrogen, by being placed between the points of two wires, which are connected with the opposite ends of a great Galvanic battery, and it appears to the spectator to burn, the phænomenon is of a different nature, and its cause may be somewhat different. Sulphur is the most negative of all known bodies, except oxygen, with respect to the metals; hence

hence too the sulphuric acid, a combination of two strongly negative substances, is the strongest, that is, the most negative of all acids, with respect to all salifiable bases. May not the appearance of fire be derived from an electro-chemical discharge? Much may be adduced in favour of this opinion from Davy's excellent investigations: it appears to me to be by no means an improbable one, and Davy himself seems to have hinted at it.

If copper bears the same relation to sulphur and oxygen that lead does, it must absorb, at its minimum of oxidation, half as much oxygen as it takes up of sulphur, that is, 12·8 or 13 per cent. of oxygen, and the sulphate of the protoxide of copper must consist of 35·83 of acid with 64·17 of the protoxide. And if the sulphuric acid requires, in the bases by which it is saturated, always half as much oxygen as it contains of sulphur, the sulphate of the oxide of copper must consist of nearly equal parts of the acid and of the base.

V. Copper and Oxygen.

A. *Oxide of Copper.*

1.) Ten grammes of copper, rolled into a very thin plate, were burnt in the muffle of an assaying furnace in a crucible of platina. The metal was changed into black oxide, and had acquired the additional weight of 1·05 grammes.

2.) Five grammes of copper were dissolved in nitric acid in a glass flask, then dried and ignited: they afforded 6·12 of black oxide of copper.

3.) Another experiment gave 6·145 of oxide.

4.) Ten grammes of copper were dissolved in nitric acid, and precipitated with neutral carbonate of potass, which had been prepared in a vessel of platina from purified tartar. The precipitate, when washed and ignited, weighed 12·33 gr. From the fluid, to which the alkali had been added, some more copper was thrown down by sulphuretted hydrogen, which, when burnt to a black oxide, weighed ·08 gr. making with the former 12·41 gr.

5.) The same quantity of copper was dissolved in nitric acid in a glass retort; the acid was carefully distilled off to dryness, and the mass left in the retort, when ignited, weighed 12·38 gr. The acid which had passed over was distilled again, and the green fluid which was left behind afforded a precipitate, by the successive addition of alkali and of sulphuretted hydrogen, which afforded ·07 gr. more of black oxide, making with the former 12·45 grammes.

It is difficult to obtain in these experiments a very accu-

 rate

rate result, because some of the copper is dissipated during combustion, or is carried away during the oxidation by nitric acid, together with the vapours which are emitted. The fourth and fifth experiments appear to be the nearest to the truth, but they require a correction which I cannot determine with perfect accuracy. It is well known that all copper contains carbon and a little sulphur. If we assume that these together make $\frac{1}{2}$ per cent. of the weight of the copper, the quantity of oxygen which the experiments indicate is so much too small, and ·05 gr. must be added to it for 10 gr. of copper, the mass of the copper being diminished in this proportion during the oxygenization. Since then 100 parts of copper, in experiment 5, received the addition of 24·5 parts in weight, we may assume that pure copper would have taken up 25 of oxygen, and that the oxide of copper, in round numbers, is composed of

Copper	80	100
Oxygen	20	25

B. *Protoxide of Copper.*

Ten grammes of oxide of copper were mixed with an equal weight of the pure copper dust already mentioned, and 75 grammes of concentrated muriatic acid were poured on them in an air-tight vessel. The mixture remained three days standing on a warm stove, and was shaken from time to time. The undissolved copper was placed on a filter, washed, and hastily dried on a plate of cast iron: it weighed 1·97 grammes. Consequently 8·03 gr. of copper had been dissolved at the expense of the oxygen contained in the oxide. Now the oxide contained also eight gr. of metal; consequently the protoxide now formed, and dissolved in the acid, contained twice as much metal as the oxide. The difference of ·03 gr. in the experiment arose probably from the operation of the concentrated acid on the copper, by means of which a little hydrogen gas was produced, which was forced out with some violence when the vessel was opened. Hence it follows that 100 parts of copper, in order to become a protoxide, take up, according to the experiment, 12·3 parts, and according to the calculation, 12·5 parts of oxygen; and the protoxide of copper consists of

consists of	Copper	88·89	100·0
		11·11	12·5

Mr. Chenevix found, in a similar experiment, the quantity of oxygen a little greater, that is 11·5 per cent. or 13 to 100 parts of copper.

If we calculate the oxygen of the protoxide of copper according

cording to the rule deduced from the experiments on lead, taking half the quantity of the sulphur, we shall have 12·8 for 100 of copper, which take up 25·6 of sulphur; and this number differs but little from the number found by direct experiments. In the analysis of the muriate of copper we shall find a new confirmation of the propriety of this mode of reasoning.

But before I proceed further in these investigations, I must determine the true proportion of the component parts of the muriate of silver, which is of essential importance for pursuing the inquiry.

VI. Muriate of Silver and of Baryta.

Rose and Bucholz have examined these salts with great apparent accuracy, and their results agree very nearly with each other; at the same time they are by no means correct. The error lies in the defective analysis of the salt of baryta. Wenzel has come the nearest to the truth of all those who have made experiments on the subject, and his researches were performed with a degree of accuracy which was not to be expected from the time in which he lived. He found in 100 parts of muriate of silver, 75·33 of silver, 6·4 of oxygen, and 18·27 of muriatic acid. Bucholz and Rose found 75 parts of silver, 7·5 or 7 of oxygen, and 17·5 to 17·75 of muriatic acid.

1.) I took three grammes of pure silver, obtained from the muriate, and kept for some time in fusion in an open fire, in order to get rid of the carbon; and I dissolved it in nitric acid, in a small glass flask; I added pure muriatic acid, and evaporated the mixture to dryness; an additional quantity of muriatic acid was poured on it, and the mass, when again dried, was melted in the flask. This colourless luna cornea weighed 3·98 gr. Consequently 100 parts of silver had taken up 32·7 of oxygen and muriatic acid, and 100 parts of the muriate contain 75·358 of silver.

2.) From 10 grammes, similarly treated, I obtained 13·275 of the fused muriate. Hence 100 parts of this contain 75·3296 of silver.

3.) Ten gr. of carbonate of baryta were dissolved in muriatic acid in a glass flask; the solution was poured into a dish of platina, carefully dried and ignited. I thus obtained 10·56 gr. of muriate.

4.) The experiment was repeated, leaving the mass to be dried and ignited in the flask; it afforded again 10·56 grammes.

Since 100 parts of the carbonate of baryta contain 78·4

of

of the earth, the 10·56 grammes of muriate must have taken up 2·72 of muriatic acid. Consequently *dry muriate of baryta* consists of

Muriatic acid	25·75	100
Baryta......	74·25	288·4

If it were possible to obtain a result for the carbonate correct within a ten thousandth, we might deduce from it in this manner a very accurate analysis of the muriate. At present the error can scarcely exceed one thousandth.

Bucholz obtained, from 84 grains of ignited muriate of baryta, 94½ grains of sulphate; this gives, if we employ his analysis of the sulphate, the quantity of muriatic acid 1½ per cent. too small; but according to my analysis of that salt, the 94½ gr. contain 62·37 of pure baryta. Now 84 : 62·37 = 100 : 74·25, which is exactly the proportion already determined for the muriate of baryta. This result therefore confirms my determination of the composition of both these substances.

5.) The 10·56 of muriate of baryta, obtained in the fourth experiment, were dissolved in water, and precipitated with nitrate of silver. The fused luna cornea weighed 14·55 gr. agreeing exactly with Bucholz's experiment, and differing but little from Rose's. Consequently 100 parts of the *muriate of silver* contain 18·697 of muriatic acid, or consist of

consist of	Muriatic acid	18·7	100·0
	Oxide of silver	81·3	434·8

The oxide of silver therefore will consist, according to this experiment, of

Silver	92·67	100·000
	7·33	7·925

VII. Sulphate of Copper.

Five grammes of neutral sulphate of copper, which had been made to crumble into pieces in a crucible of platina, heated to the temperature of melting tin, were dissolved in water and precipitated by muriate of baryta. The precipitate, washed and ignited, weighed 7·22 grammes, indicating the presence of 2·455 gr. of sulphuric acid; consequently the remaining 2·545 of the 5 gr. were oxide of copper; and the sulphate of the oxide of copper consists of

Oxide of copper	50·90	103·66
Sulphuric acid	49·10	100·00

Supposing 100 parts of sulphuric acid to require in the base, by which they are saturated, 20·29 of oxygen, this quantity must be contained in 103·66 of the oxide of copper. Now 125 : 25 = 103·66 : 20·73; so that the result of calculation

calculation differs very little from the analysis: 100 parts of the acid ought to combine with 101·45 of the oxide; and the difference may perhaps depend on a portion of water left behind in the salt. It appears also that in this sulphate 100 parts of metallic copper are united to 50 of sulphur, which is very little less than twice the least quantity that copper takes up in the form of a sulphuret.

I imagined also that the subsulphate of copper, already known, must contain copper and sulphur in the proportion determined by this experiment. I precipitated, in order to examine this, a solution of the sulphate of copper by means of caustic ammonia, so that the whole of the copper was not thrown down. The subsalt was washed and dried on a filter, and gently ignited. When dissolved in nitric acid, and precipitated by nitrate of baryta, it appeared to consist of 20 parts of sulphuric acid, and 80 parts of oxide of copper. Consequently the acid was combined, in this subsalt, with nearly four times as much of the base as in the neutral salt, and 100 parts of copper take up, in this case, only half as much sulphur as in the sulphuret of copper. Hence it seems probable that copper and sulphur must exist in the required proportion in the sulphate of the protoxide of copper: of this salt, however, I am unacquainted with the characters and with the mode of preparation; its composition may however be calculated in two ways, which afford nearly the same result. In the sulphuret of copper, if 100 parts of copper take up 25 of sulphur, 125 parts of this substance must give 173·86 of sulphate of the protoxide, and 100 parts of sulphuric acid will be combined with 183 of the protoxide. We shall find hereafter that 100 parts of the muriatic acid are saturated by 278·4 of the protoxide of copper; and since they are also saturated by 288·4 of baryta; and since 100 of sulphuric acid combine with 194 of baryta, we have 187 for the proportion of protoxide of copper answering to 100 of sulphuric acid; for, 288·4 : 278·4 = 194 : 187; and this number differs but little from 183, the result of the former calculation.

VIII. Muriate of Copper.

I have advanced the conjecture, that every other acid, as well as the sulphuric, in order to be saturated by a base, requires the same quantity of oxygen to be contained in it: and in order to examine the truth of this opinion, I fixed on the muriatic acid.

A. *Muriate of the Protoxide of Copper.*

A solution of this substance in concentrated muriatic acid

acid was precipitated by boiled water, and the precipitate well washed with boiling water; it was pressed on a filter, hastily dried on a hot brick, put into a small glass retort, and melted by a red heat. Of this fused substance six grammes were dissolved in pure nitric acid, and precipitated with nitrate of silver. The precipitate weighed, after fusion, 7·12 gr., which implies the presence of 1·321 gr. of muriatic acid. This muriate consists therefore of

Muriatic acid	26·42	100·0
Protoxide of copper	73·58	278·4

Hence the quantity of the protoxide which saturates 100 parts of muriatic acid contains 30·93 of oxygen; for 112·5 : 12·5 = 278·4 : 30·93.

B. *Muriate of the Oxide of Copper.*

Four grammes of black oxide of copper were dissolved in muriatic acid, and carefully dried, so as to drive off the superfluous acid. Hence was obtained a bright liver-coloured mass, which recovered, when exposed to the air, its water of crystallization, and its colour, [becoming green as at first, Kem. II. 331. Engl. Tr.] The salt was dissolved in water, and precipitated with nitrate of silver. The muriate, thus formed, weighed, when fused, 14·4 gr., answering to 2·69 of muriatic acid. We have therefore for the muriate of the oxide of copper,

Muriatic acid	40·21	100·0
Oxide	59·79	148·7

If we calculate the result of this experiment from the analyses of the sulphate of baryta, of the sulphate of copper, consisting of 100 acid and 101·8 oxide, and of the muriate of baryta, we obtain 194 : 101·8 = 288·4 : 151·3, that is, 2·6 more of the oxide than the experiment exhibits.

In this experiment 100 parts of muriatic acid require 30 of oxygen in the base by which they are saturated; for 125 : 25 = 148·7 : 29·74, or according to the last calculation, 30·26, which differs but little from 30·93, the result of the former experiment. I consider therefore this experiment as an additional proof, that the oxide of copper contains twice as much oxygen as the protoxide. And that the protoxide of copper, which saturates a given quantity of the muriatic acid, must contain the same quantity of oxygen as the oxide which is capable of saturating it, is placed beyond all doubt by the mode of preparation of the salt of the protoxide.

C. *Submuriate of Copper.*

A solution of the neutral muriate of copper was precipitated

tated by caustic potass, so that the whole of the copper was not separated. The mucilaginous green precipitate was washed on a filter with boiling water; but since the water passed through it too slowly, after the filtration had continued two days, it was dried, powdered, and then boiled with spirit of wine. The salt was again well dried, and became of a yellow brown colour. I introduced five grammes of it into nitric acid, in which it was very slowly dissolved: it was then precipitated by nitrate of silver. The luna cornea, when fused, weighed 3·3 gr., indicating ·617 of muriatic acid. The liquid was boiled with mercury, in order to separate the oxide of silver, then evaporated in a crucible of platina, and ignited; it afforded 3·680 gr. of black oxide of copper. The subsalt consists therefore of

Muriatic acid	14·36	100
Oxide of copper	85·64	596

Consequently 100 parts of muriatic acid are combined, in this salt, with four times as much of the base as in the neutral salt; for $148 \cdot 7 \times 4 = 594 \cdot 8$, which differs only by 1·2 from the result of the experiment.

[To be continued.]

XXXII. *On the Removal of Impediments to the Acquirement of Vision by Persons cured of Cataract.*

To Mr. Tilloch.

Sir,—In a work recently published, on Diseases of the Eye, by Mr. Adams, Oculist Extraordinary to His Royal Highness the Prince Regent, and late Surgeon to the West of England Infirmary for curing Diseases of the Eye, instituted at Exeter, I have perused with much satisfaction some original observations on the causes, and ingenious hints for the removal, of the impediments to the acquirement of vision by persons cured of cataract who were born blind with them. This form of the disease appears to be much more frequent than is generally supposed, if we may judge from the large number of persons (upwards of seventy) Mr. Adams mentions to have successfully operated on. The benefit however to be derived by the patient even after the most successful operation, according to Mr. Adams's account, who seems to have bestowed great attention to this subject, depends on their after education. This then must be considered an object of the highest importance; and as I know of no one except himself who has hitherto suggested any plan for the purpose, I inclose a copy

copy of his observations on the subject, for insertion in your Journal, believing they will be perused with much interest, and I think it not improbable that Mr. Adams may be assisted by some one of the many ingenious readers of your valuable publication, in his laudable endeavours to establish a system for the education of persons of the description he mentions.

I am the more sanguine in this expectation, when I consider what has been accomplished by the benevolent and indefatigable exertions of the Abbé de L'Epée, on subjects who at the commencement of his experiments must have afforded very different prospects of success.

I have the honour to be, &c.

W. B.

Extract.

"The advantages of an early operation in patients affected with congenital cataract have been slightly noticed. This is a subject of the highest importance; for those who have had the disease removed at an advanced age, are equally destitute of a knowledge of visual objects as the merest infant, while at the same time they are placed in circumstances far more unfavourable for its acquisition. The healthy infant examines every object with the eagerness natural to its age; while the more aged congenital patient, from long-continued habit, has contracted a disinclination to the exercise of the eyes, which he is seldom able entirely to overcome. The rolling motion of the eye, depending on an involuntary action of the muscles, is thereby extremely difficult to be corrected, when the removal of the cataract has been delayed, and it affords another obstacle to improvement in vision: this points out the necessity of an early operation. My own experience as well as that of Mr. Saunders, and my late colleague Mr. C. T. Johnson, sufficiently demonstrate that it may be performed as soon after birth as the defect is discovered, with the most perfect ease and safety*. Were it practicable, I would not suffer an infant's eyes to be exposed to the light till the cataracts were removed; by which means I conceive the involuntary action of the muscles of the eye-ball might be in a great measure, if not wholly, prevented. Most authors who have written on the subject of congenital cataract,

* Mr. Saunders cured an infant of congenital cataracts by the posterior operation, at two months old; Mr. C. T. Johnson performed the same operation with success at six; and I have been equally fortunate on a child of ten months old.

mention

mention the imperfect vision of patients for a longer or shorter period after the operation, and attribute it to an original deficiency of the retina itself. None of them, however, appear to me to attach sufficient importance to the subject, except the late Mr. Saunders, whose opinions, as expressed in a letter written a short time before his death, correspond more exactly with mine than any others I have seen. It is evident, from the powerful obstacles to the acquisition* of useful vision, that unless congenital cataracts are removed during the earliest periods of infancy, the progress in the knowledge of visual objects must be very slow and tedious. This is indeed sometimes so much the case, that I have known instances where both the patient and his friends have despaired of ultimate success, and have altogether ceased to make the necessary efforts, even after the patient had begun to see objects with tolerable distinctness. It is by no means uncommon for the friends of a congenital patient to expect that he should obtain the power of perfect vision immediately after the operation, and even attribute their consequent disappointment to its imperfect execution. Parents ought therefore to be fully apprized of all these attendant circumstances of the complaint, and of the great necessity of a regular and constant attention to the future education of the patient; and they should not be discouraged because immediate success does not attend their most anxious efforts. From an early period of my attendance at the London Eye Infirmary, my mind has been deeply impressed with the conviction, that much more than is generally supposed necessary, remained to be accomplished after the removal of the disease; and every day's experience confirms me in this opinion. Since an extensive practice has opened to me a wider field for observation, I have directed a considerable portion of my attention to the development of the various causes which retard the patient's progress in acquiring a knowledge of visual objects, as well as to the best methods of training the eyes for its attainment: and I am convinced that if proper plans, which must vary according to the ca-

* "To turn the faculty of sight to use, so as to display precise notions of objects, demands experience, which can only be given by the exercise of vision with considerable attention for a long time. The operation has no power to confer actual knowledge of objects. It only prepares the eye for receiving, and afterwards the intellect must be employed on the objects so received, before any readiness can be acquired. The child therefore must be the object of the parent's attention, and be regularly and diligently exercised about large objects at first, and be taught to know them; then with smaller, and so on by degrees."—Vide Saunders's Posthumous Works, p. 155—62.

pacity

pacity and disposition of different patients, were systematically pursued; not only would useful vision be obtained by congenital patients in a much shorter period than usual, but it would fall little short of that enjoyed after the removal of the disease from persons not born blind. An intelligent person should always be appointed to superintend the management of those cured of congenital cataracts, whose sole business should be to watch and correct as much as possible those habits which impede the acquirement of vision, and to assist, by every expedient which ingenuity can devise, in the attainment of the desired object. To correct the rolling motion of the eyes, and to acquire the power of keeping them steady, the patient, after being fitted with spectacles, should be made to look steadfastly on one object. The muscles of the eye, and the organ itself, will soon become fatigued with this exercise; but it should be daily continued at proper intervals, by which the power of fixing the eyes at pleasure will rapidly increase. He should be made also to pick up small objects, such as grains of rice, &c.: this is also particularly useful, as it will in time enable him to judge accurately of distances, of which at first he is ignorant.—Letters of a large size should be next cut out on pasteboard: as these are capable of being examined by the touch as well as sight, they will begin to afford him a knowledge of different forms and shapes. The propensity to indolence and want of exertion in congenital patients, even in children, is often so great, that the preceptor will have considerable difficulty in making them apply daily for as long a time as is necessary; and I have always found it regarded more as a task than a pleasure: but this must not tempt him to any relaxation in the system.

"The sensibility of the retina to the impression of light increases in proportion to the degree of exercise to which the eye is subjected *. It is therefore obvious that the de-

* This appears very strikingly to have happened in the case of Mr. Purkis, organist of St. Clement's Church, Strand, who was born with cataracts, and was operated on at thirty years old; before which he was only capable of perceiving light and brilliant colours. In a note at the end of his case Mr. Adams states: "The professional avocations of this patient, and the continual rolling motion of his eyes, have hitherto prevented him from reaping all the benefit to be derived from the operation. The rolling motion of his eyes, however, is very much corrected, and he has acquired the power of fixing them at pleasure. He has learnt to read musical characters, to tell the hour with the greatest accuracy on a watch-dial, or by a church clock at a considerable distance, and his power of vision continues to improve. Since his return from Exeter, he walks without a guide by day and night, which he never ventured to do before the operation, and has now entirely laid aside the use of spectacles, except to view minute objects."

fect

fect in its function arises from continued inaction, and can therefore only be cured by constant application. Those who possess the blessing of sight, are in the constant and almost momentary habit of exercising the retina, while in a congenital patient all its powers are suffered to lie dormant. Even in persons not born blind, who have successfully undergone the operation for cataract, if the disease had continued for many years, the functions of the retina during this period have been much impaired, and are afterwards materially improved by exercise. If this partial want of a natural sensibility in the retina be confined to one eye, which is often the case in the slate-coloured cataracts with transparent edges, the other eye should be covered, or the spectacle before it obscured, to prevent the passage of light, while the one affected is exercised as much as possible.

"In the hope of establishing a systematic plan of educating persons who have been unfortunately affected with congenital cataracts, a young gentleman about 14 years old, on whom I successfully performed the operation, has been placed under the tuition of an able and ingenious master, who has been made acquainted with the different causes which appear to me to retard the acquirement of vision, and with the means judged necessary to be employed in his instruction. The progress this patient made while he was previously with his friends, by an attention to some of these rules, leads me very sanguinely to anticipate the greatest success from the present experiment. After his recovery from the operation he could merely distinguish colours, but was so entirely ignorant of the forms of objects that he could not perceive any difference between a square and a circle. To my great gratification, when he arrived in London, nine months after the operation, he could read letters of a middle size, could help himself at table without assistance, walk alone in the street, &c.; and I have great reason to hope, that at some future period I shall be able to lay before the public the favourable result of my efforts, assisted by those of the intelligent master on this interesting subject. On the contrary, I have frequently seen instances where the operation has been attended with equal success; yet, owing to a want of a proper attention afterwards, the patient has derived little or no benefit from it. A young lady upwards of twenty years of age, one of the first persons I operated on at Exeter, to whom I was enabled myself to pay at first a good deal of attention, a month after she was cured, could distinguish

the minute marks on a watch-dial, and see a hair when plucked from her head; but so great was her indolence, that it was only by constant watching she could be urged to any kind of application; and I have learnt that since her return home she has entirely given up every exertion for the improvement of her sight, and now remains nearly as helpless as ever. The little attention which appears hitherto to have been paid to a subsequent education, in the constant exercise of the improving powers of vision, and the wish of impressing its importance still more strongly on the minds of those who are not sufficiently aware of the prevalence of the retarding causes, have induced me to extend these observations; and I feel a strong hope that, by the hints which they contain, they will materially assist the efforts of many anxious parents in the future education of such of their children as have undergone the operation for congenital cataract."

XXXIII. *An Essay on the medical Effects of Climates*.*

A COMPLETE system of meteorology, even so far as the properties of climates, with regard to temperature only, are concerned, presents almost as great difficulties as a complete theory of the nature and cure of diseases. In this, as in many other departments of medical knowledge, we perpetually find a multiplicity of accounts, apparently well attested, but totally at variance with each other, which render it desirable to appeal to some more satisfactory testimonials than the results of common and superficial observation; while the evidence, which would be required for forming useful conclusions, upon safe and scientific grounds, although in this case completely within the scope of the human faculties, is still such as to require, for its production, a combination of perseverance and accuracy, which has certainly never yet existed, and which probably can scarcely ever be expected to be found in a sufficient number of collateral observers. Any voluminous work on the subject, whether systematic or empirical, must unavoidably contain much useless and some erroneous matter; and a short statement of a few facts, which appear to be tolerably well ascertained, first, respecting the physical characters, and secondly, respecting the medical effects of the principal climates which deserve our notice, is all that it will be possible to attempt in the present essay.

* From Dr. Young's Introduction to Medical Literature, Lond. 1813.

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The simple indications of a thermometer, however accurately they may be observed, in the most unexceptionable exposure, by no means afford a correct test of the temperature, as it affects the human system: nor is it possible to express the modifications produced by wind and moisture, even supposing them to be easily known, by any numerical measure which shall be applicable to every relative situation of the individual. I have known an atmosphere at 65°, with a thick fog, and a very little wind from the N.E., appear, to a person taking moderate exercise, most oppressively sultry; although a person, sitting long still, might have felt the same air uncomfortably cold. Moisture must make both heat and cold more sensible; the one, by diminishing perspiration, the other, by increasing the conducting power of air. Wind is doubly concerned in affecting the properties of a climate; first, as the great cause of preventing a general accumulation of heat over considerable tracts of country; and secondly, as having a similar effect with respect to the immediate neighbourhood of the person; and its operation is as generally perceptible in the latter way, where we have no precise mode of estimating its magnitude, as in the former, where it is correctly indicated by a thermometer sufficiently exposed: although, in fact, the most shaded fixed thermometer may often be observed to indicate a temperature many degrees higher, than that of the breeze which is circulating in the neighbouring country. Still more commonly by the sea side, the wind exhibits the temperature of the water over which it has blown: at Worthing it is seldom above 64° in the hottest weather, although the sea, when the tide flows in at noon, over the heated expanse of sand, is sometimes raised to 78°, where it is several feet deep.

To the inhabitants of these islands, the most important properties of the climates of other countries are those, which render them more or less fit for the residence of persons liable to catarrhal or consumptive affections. Hence, warmth and equability of temperature, especially in the winter months, are the first objects of our inquiry in the theoretical comparison of climates. Moisture is supposed, by some, to be favourable, by others, to be unfavourable, to such persons: it may therefore be safely neglected, except as tending to increase the evils depending on a want of equability of temperature. The effluvia of moist ground are sufficiently well known as the causes of paludal fevers; further than this they require no particular investigation. Nor can we attempt to assign any reason for peculiarities, which render

some situations preferable to others, for some individuals only, labouring under a given disease, as asthma; which is sometimes induced by the atmosphere of cities, and sometimes of the country; and which is occasionally mitigated by a residence in places having no marked distinctions from such as are less favourable to it, as Kensington, and perhaps some others.

In the hotter seasons, there are few diseases, and few constitutions, which would require a climate milder than our own: in the colder, an increase of the facility of circulation, which heat appears to afford, may often be beneficial, partly perhaps as exciting perspiration, and partly as preventing too great a congestion of blood in the internal parts of the body. The mean temperature of the six winter months is therefore the first point of comparison, that requires our attention, and such a comparison may easily be derived from the registers, which are usually kept in circumstances nearly similar.

From October to March.

London, R. S. 1790–4	43·5°
Edinburgh	40·4
Dawlish, Sir W. W. M. S. 1794 (Lond. 44·1°)	45·3
Ilfracombe, without doubt incorrect	(55)
Paris	41·2
Lisbon	55·5
Malta, Domeier	63
Madeira, Gourlay. (S. W. aspect, M.)	63
Bermudas, M. S. R. S. 1790	68
Jamaica, Botanic garden at Kingston, Clarke, Dunc. med. comm. vii. 369	74·5

From November to March.

London, 1808–9	42·6°
Penzance, 1808–9, Stirling, at 10, or about 1° above the mean	48·1

From January to March.

London, 1809	43·1°	(Jan. 37·9°
Glasgow, 1809, Stirling, at 10	40·3	33·1
Penzance, 1809, Stirling, at 10	48·5	46·7 (Dec. 43·7°)
London, 1790–4, 8 or 7 and 2	41·6	39·1
Sidmouth, 1800, M. S. R. S. 8 and 2	41·7	42·3)

February and March.

London, 1803, 7 and 2	41·5°
Clifton, 1803, 8 and 2. Carrick	42·5

From

From October to December.

London, 1811, mean of extremes in each month	47·0°
Sidmouth, 1811, Clarke	45·7

From December to February.

London	39·7°
Edinburgh	36 7
Paris	36·8

It appears from this comparison, that none of the situations here enumerated, North of Lisbon, except Penzance, has any material advantage over London in the mildness of its winter. The best parts of Devonshire seem to be about a degree and a half warmer; Torquay however may perhaps be a little milder than this; the account, which was kept at Ilfracombe, must have been taken from a thermometer in a confined or a sunny situation. But Penzance may be fairly considered as having a temperature $4\frac{1}{2}°$ higher than London in the coldest months; nor is the journal here employed the only one, which allots such a superiority to the climate of this extremity of our island. It is remarkable, that the temperature of the three coldest months is the same at Paris as at Edinburgh, being, in both these cities, about three degrees lower than in London. There are probably particular spots on the coast of Hampshire or Sussex, which, from their sheltered situation, must be considerably less subject to the effect of the northern and eastern winds, than most other parts of the island; and Hastings, or its neighbourhood, may perhaps be reckoned among the most eligible of these; but the further we go up the channel, the more remote we become from the mild gales of the Atlantic, while the prevalent south-westerly winds, in passing over a considerable part of the continent, must have lost much of their warmth. It is scarcely necessary to observe, that both Malta and Madeira present, numerically, a mean temperature for the winter months, as favourable for an invalid as can possibly be desired.

Equability of temperature is a second quality, of no small importance, as tending to diminish the chance of incurring, or aggravating, pulmonary diseases, by repeatedly taking cold. When, indeed, the temperature is much below 60°, the most material changes are those which occur upon going from the house into the open air; so that a cold climate becomes, in some degree, of necessity a changeable one also. The regularity of this change, and the power of avoiding its effects by additional clothing, as well as of obviating them in some measure by exercise, contribute

however to lessen its influence; and it does not therefore altogether supersede the effects of that changeableness, which consists in a great extent of variation of the temperature of two successive days, or of different hours in the course of the same day. The simplest, and perhaps the best mode of appreciating the effect of the extent of such a variation, in deteriorating a climate, is to observe, for each month, the greatest variation, at the same hour, in any two successive days within its duration. The mean variation of successive days may also be computed, in order to assist in the comparison; and the mean diurnal range, or the space through which the surface of the mercury moves, in ascending and descending, throughout the day and night, will give a collateral estimate of a similar nature. The best practical mode of deducing this range from the observations is, to find separately the mean of the heights for the morning and afternoon, and to double their difference. Where none of these particulars can be obtained, the extreme variation of each month will afford a character not altogether unimportant.

[To be continued.]

XXXIV. *Notices respecting New Books.*

Mr. Farey's *Mineral and Agricultural* "*Report on Derbyshire.*"

The *first* chapter and volume of this work have been some time before the public: in our xxxixth volume, pages 192 and 253, we gave extracts from this volume, and made references to three previous communications from Mr. F. inserted in previous volumes, of matters contained therein, though under different forms of arrangement. We have now to notice a *second* volume of this important work, containing chapters 2 to 13, treating of the subject usually embraced by the County Reports of the Board of Agriculture; but treated most of them, as appears to us, with a degree of precision in all matters relating to persons, improvements, places, *soils*, *strata*, &c. which has not, we believe, been attempted in any of the other reports, though their utility depends so much on such particulars.

In the chapter on woods and plantations, we find the subject of *pruning* and training up young plantations ably treated, and at considerable length, as one which previous reporters had rather surprisingly overlooked, and the suggestions of the author seem to us calculated to obviate, in time,

time, the alarming and increasing scarcity of large oak timber for naval purposes.

In the section on *draining*, several instances are mentioned of unsuccessful attempts at improving lands, by Mr. Elkington, in Derbyshire, and in Bedfordshire, for the late Duke of Bedford, while the author was his Grace's land steward; from which it would appear, that far less of science or success attended Mr. E.'s practice as a drainer, than the public have been led to believe. The principles of draining seem now, however, to be well understood; the art is successfully practised by great numbers of professional drainers all over the country, and scarcely any thing seems wanting in point of theory, to Mr. John Johnston's able work on this subject, published by the Board. Some omissions and misstatements in Mr. Batchelor's Bedfordshire Report relating to this and some other branches of rural improvements, in which the author was concerned, are noticed in this volume.

The still mysterious operation of *lime*, as a manure or stimulant to land, may perhaps at some future period receive helps towards its elucidation, from the great pains which the author has taken in this volume, to ascertain the stratum and quarry, from whence the lime was procured, in all the numerous instances which are mentioned of its use.

In the chapter on *irrigation*, it seems pretty satisfactorily made out, that the *flatness* of the watered surfaces, of meadows in Derbyshire, Norfolk, Bedfordshire, and other places, have principally occasioned the numerous failures which have been complained of, in the practice of this art, though charged by Mr. Batchelor, and many other writers, to the account of *soils* and *waters*, either as to the mineral qualities or alleged coldness of the latter: some extensive schemes of irrigation in newly inclosed parishes in Bedfordshire, belonging to the late Duke of Bedford, are mentioned and shortly described.

On the whole, we can recommend this volume to the careful perusal of our agricultural readers, as containing much practical and valuable information, applicable greatly beyond the limits of the county whose name it bears, and in the arrangements of which much pains seem to have been bestowed. Excellent indexes accompany this and the former volume.

Mr. CLARK's *Dissertations and original Experiments on the Foot of the living Horse, exhibiting the Changes produced by Shoeing, and the Causes of the apparent Mystery of this Art.*

[Concluded from p. 47.]

The extension of science, humanity, and interest, all combine to give importance to Mr. Clark's discoveries, and render it necessary to make them better known to the public. The author well observes, in defence of the veterinary profession, that "there is no art so perplexed and difficult that by human industry and research, steadily and properly exerted, cannot be rendered more clear and practicable." He might have added that the progress of animal is quite equal to that of human medicine, although the latter interests all, the former only a part of mankind. Mr. C. however, has given another, and, with his professional brethren, very rare example, that of uniting the urbanity of the scholar and the precision of the philosopher in writing on veterinary practice. Formerly the vocabulary of such writers was entirely vituperative. "It is indeed," says Mr. C. "high time the wretched style of declamation and abusive writing on these subjects should give way to a better taste, that of real investigation and research, as in other objects of a scientific nature, by which alone the art can receive any useful accessions, and mankind and the horse be benefited. The empty verbosity of style alluded to began about the reign of Charles II. or a little earlier, and has continued with few exceptions ever since. It was unknown before that period, and was in reality the natural produce and legitimate offspring of jockeyism and the race-course."

The want of scientific terms, and the literal absurdity of many of those now in use, imposed on the author an arduous task to make himself intelligible. Such is the word *heel*, when applied to the foot of a horse, where there is really nothing analogous to the human heel. To remedy this inaccuracy, Mr. C. has used the expressions "lower or horny heels for the parts covered by the shoe; the posterior heels, or back of the frog, and the superior or heels above the hoof, formed by the cartilages." In like manner he shows that the wall of the hoof, as the exterior horn of the foot is called, is not conical but cylindrical, being a very obliquely truncated cylinder. The frog, which is a triangle of elastic horn, has the effect of and may be compared to an elastic key-stone received into an elastic arch. This structure,

structure, destroying the continuity of the horny circle, contributes to divide equally the pressure on all parts of the hoof. The cleft of the frog, which keeps the foot from slipping, and also from too great condensation of the horn by pressure, is prevented from rupturing by a stout cone of horn passing directly from it into the sensitive frog. This cone is quite as hard as the exterior horn, and thus obviates the tendency to division which exists in the horse's foot as well as in cloven-footed animals. The destruction or rupture of this cone becomes the source and cause of the disease called the *running thrush;* it splits or cracks from whatever cause: the consequence is that extraneous substances introduce themselves, and these are followed by ulceration and discharges of matter. Mr. C. traces the various appellations which the frog has received in different nations. The Latins call it *furca,* the French *la fourchette,* the fork; and once in Vegetius it is called *pendiginem,* apparently from its hanging from the roof of the sole. The Italians have no proper name for this part, as *pastoja* is the pastern, and *tuello,* or root of the nail, precisely suits what the author calls a "coronary frog band;" a circle enveloping the upper part of the hoof adjoining the hair. The Spaniards call it *ranilla* or little frog, but the Portuguese have no distinct term for it. The Greeks termed it χελιδων or swallow. The origin of the terms *running thrush* is thus traced: "*Furca,* in French *fourche,* and its diminutive *fourchette;* this contracted became the *running fourche;* and thence we find about the days of Elizabeth, as in Blundville and other writers of that period, *running frush;* and subsequently in James's reign, on the establishment of horse-races, and the prevailing influence of the jockeys, who not finding in their vocabulary of English words such a one as *frush,* declared it must mean a *thrush;* and a *running thrush* it has ever since been called by the whole kingdom."

The horny heels, sole, wear of the hoof, and its bearings on the ground, are all examined with care, and many important facts and circumstances are suggested, which we hope will not be overlooked by the thinking part of horsemen. But the first and leading fact is the unrestrained growth of the hoof, which requires five years before being iron-bound, instead of two or three as is too common. "The horse," says the author, "like other large animals, is slow in acquiring maturity, and like them is not very short-lived. Some celebrated writers have considered the natural period of his life about fifty years. This was before

fore the art of shoeing commenced, and may be not far from the truth in those times. If we were to give an opinion on this matter; we should state it as our belief, that he acquires his stature or height at about five years, but obtains his full bulk and strength about the eighth year; and this period, as in most other animals, if multiplied by four, will give somewhere about the period of his natural life; which, without any desire of unnaturally extending, would be from 32 to 40; and at the former age we have seen (setting aside the state of his feet) horses capable of a great deal of service. But frequent visits to the slaughter-house led us to observe and conclude, that six arrive there before, to one after the 14th year!" Thus, it appears that men abridge the lives of horses on an average, just to one third their natural duration. The unfortunate animals are crippled with strait shoes, their feet are contracted as unmercifully as those of the Chinese females, and it becomes œconomy to "use them up," or wear them out by the greatest barbarity, and then sell them to the caterers for dogs and cats! The most obvious evil of the iron-shoe is "its permanent application and constant pressure against the bottom of the foot, with a force altogether indefinite, depending on the strength with which the nails are clenched, and the proximity of the shoe to the sole, which causes it to act with more or less violence against the lower surface of the coffin-bone. Next the nails in the sides, being immoveably blocked in the perforations of the shoe, create a solid resistance of iron at this part, not admitting the natural expansion of the hoof; and it must be obvious that they almost, though not entirely, prevent, by keeping the quarters fixed, every movement of the posterior parts and heels." It is impossible to form any adequate idea of the dreadful tortures which the animal must experience under such circumstances; it is also necessary to examine the author's very accurate plates, to comprehend the extraordinary changes which shoes make even on the very bones of the horse's foot. Mr. C. took casts of natural hoofs, from which that ingenious and accurate delineator Mr. S. Edwards made drawings, which correctly represent the figures of the hoof in its natural state, and after it has undergone the process of shoeing at different periods. In this case the outline of the hoof is changed from nearly a circle to a good oval, the posterior part contracting, the toe extending. That part of the hoof which is nearly horizontal can grow a little, but that which is perpendicular cannot possibly overcome the resistance of a thick bar or iron.

In

In an "Essay on the feet of horses that have suffered by shoeing, with experiments exhibiting the effects of a sudden removal of the shoes and turning to grass," we find some very curious osteological and physiological remarks, which almost make us doubt the accuracy of our anatomical knowledge of the horse. We allude to what the author calls a "*patiloba* or *scaly node* of the coffin-bone," which consists of plates, or scales, forming an oblong lobe of some extent at the hind part of the foot, and placed over one another like tiles, but not in contact, having spaces between them. This peculiar bone, it seems, is entirely destroyed by tight shoes. The numbness occasioned by the pressure of the nails also makes horses cut in travelling. The conclusion which Mr. C. draws from his experiments, is, "that after the foot has been exposed a certain time to the operation of the iron, it becomes so much changed from its natural state, that it is safer and more advisable for it to remain in the diminished and fixed condition to which it is reduced, than by any measures, especially severe or coercive ones, to attempt its restoration; as any sudden or violent change appears to disturb the foot and bring on morbid affections, rather than the healthy condition of the part; so that a continuance of it in this state appears the less evil, or even an advance of the mischief, if it be a very slow and uniform one, is to be preferred. Such appear the disclosure and unfolding of this mysterious matter, and which, though it may appear simple when explained, has been no small stumbling-block to many people both in and out of the profession. The exposure of strong feet a few days or weeks at grass, merely to cool them or remove any casual compression from nails, is not prohibited by this caution, but further exposure would be injurious. The author has the pleasure of finding his discovery immediately reduced to practice, and several noblemen and gentlemen now bring up their young horses in their parks and pastures without having recourse to the early and unnatural application of iron shoes. Even the young horses for the regiments of Guards are kept in this state of nature, in consequence of which their lives will in all human probability be doubled.

The "Essay on the knowledge of the ancients respecting shoeing" is more interesting to the lover of polite literature, and more entertaining to those who can amuse themselves with the natural history of language and the arts. The epithet "brazen-footed," χαλκοποδες, used by Homer, he shows, does not mean that Neptune's horses were shod with

with brass, no more than Isaiah's hoofs like flint, were of that stone. The εμβαται and καρβατιναι of Xenophon were covers for the legs and feet of both soldiers and horses, made of skins or leather. According to Aristotle, the camel was subject to tender feet on going long journeys, when it was shod with the *karbatinai*. It appears that the earliest veterinary surgeons were those employed by Constantine to attend the horses of the Roman armies. Apsyrtus, whose work on this subject is still extant, lived at that time. When iron shoes were introduced, about the fifth century, the smiths became the horse-doctors, and were called *ferrers;* afterwards *ferriers,* and corruptly *farriers,* from *ferrum,* iron. Instead of leather socks, some think the ancients used broom twigs; but it appears better ascertained that they sometimes put iron over the leather. It is more probable that the broom was applied rather medicinally, than to protect the feet as shoes. Even the Spanish broom (*spartium junceum* Linn.) would be very inadequate; the barilla-matting of the *stipa tenacissima* Linn. would be much better, and also the leaves of the dwarf palm, *chamærops humilis* Linn. The origin of modern shoeing can be traced no higher than the nailed shoe found in the coffin of King Childeric, who died in 481; but a more unequivocal intimation of the modern horseshoe occurred in the ninth century under Leo X. of Constantinople. Daniel says that horses were shod only in frost or particular occasions in that age. William the Conqueror introduced the practice of shoeing into England; he gave Northampton to Simon St. Liz, a Norman, to provide shoes for his horses; and Henry de Ferrers, the ancestor of Lord Ferrers, was the superintendant of the shoers.

From these extracts, the reader may learn that Mr. Clark has introduced more original and curious information than could naturally be expected on the uninviting subject, the feet of horses. Whether these Essays be considered as the work of a man of science, learning, or philanthropy, they do equal honour to his head and heart, and are highly worthy the serious attention of all men who have ever been conveyed from place to place by a horse.

Thomas Myers, A.M. of the Royal Military Academy, Woolwich, author of a Compendious System of Modern Geography, historical, physical, political, and descriptive, intends soon to publish, elegantly printed on a large sheet, a statistical Table of Europe, uniting all that is most interesting in the geography of that distinguished quarter of

the

the globe, and showing at one view the territorial extent, the military strength, and the commercial importance of each state.

Dr. Brewster, of Edinburgh, is about to publish a Treatise on new Philosophical Instruments for various purposes in the Arts and Sciences, with Experiments on Light and Colours, in one volume 8vo. with twelve Plates.

Mr. Thomas Forster has now in the press Meteorological Researches and Journals, with Engravings, 8vo.

XXXV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

In our report of last month, our account of Dr. Brewster's paper was in some respects inaccurate; the following is more correct:

This paper contained a short abstract of Dr. Brewster's numerous experiments on light and colours, and was divided into four parts. 1. On a new property impressed upon light by transmission through the agate. When light is transmitted through a thin plate of transparent agate cut in a direction perpendicular to the laminæ, Dr. Brewster has found that it possesses all the characters of one of the pencils formed by double refraction, one of the image of the luminous object from which it proceeds disappearing in every quadrant of its circular motion when viewed by a prism of Iceland spar. The kindred substances of carnelian and chalcedony possess the same property. 2. On the double refraction of chromate of lead. Dr. Brewster found that this metallic salt possessed a double refraction, which was nearly thrice as great as that of Iceland crystal. 3. On substances which have a greater refractive power than the diamond. Since the time of Sir Isaac Newton, who first discovered the powerful action of the diamond upon light, it has always been conceived that this precious stone possessed the highest refractive power of all bodies whatever; but Dr. Brewster has found that both chromate of lead and realgar exceed it considerably in refractive power. 4. On a double dispersive power in doubly refracting crystals. Dr. Brewster has discovered, that all bodies that possess a double refraction have also a double dispersive power, one of which is very much greater than the other; the highest dispersive power of Iceland spar being considerably

derably above that of water, while the lowest dispersive power is very much below that of water.

February 25. In consequence of the continued indisposition of the President, the Earl of Morton, Vice-president, was in the chair, when a paper by Dr. Wollaston was read, containing a description of his newly invented single lens micrometer. This instrument is made like a common telescope, but the focus of the lens is only 1-12th of an inch: this glass is placed behind a brass plate, through the centre of which an eye-hole is drilled; the subjects to be viewed are placed between glasses serving as object glasses, and the measure of the magnifying powers and of the subjects examined is taken by means of a certain number of wires fixed near the object glasses. The measure and number of the wires being determined, the objects may be extended to such distances as to give their dimensions by making a wire the two hundredth part of an inch cover them. The description was illustrated by designs of the micrometer, which the author adopted in consequence of his experiments on drawing very fine wires, some of which did not exceed the thirty thousandth part of an inch; but they were incapable of supporting themselves at this fineness, and were broken in very short pieces. He found wires 18000 of which covered an inch, to be the finest and strongest for any useful purpose.

A paper by Dr. Pearson, on the tingeing matter of the bronchial glands of the lungs, and on the black or tingeing matter of the lungs themselves, was read. From his researches it appears that this black matter is principally charcoal in an uncombined state, or at least that it is only intimately mixed with a small portion of animal matter. He conceives that it is derived from the atmosphere in breathing; that it is first conveyed into the air-tubes, and from them by means of the numerous lymphatics into the bronchial glands, and therefore that it is not a secreted substance. This subject being so novel, Dr. Pearson declined entering into much reasoning or drawing many conclusions until more facts are brought to light.

March 4. Earl Morton in the chair. A letter from the late Mr. W. Kirby Trimmer to the Right Hon. Sir Joseph Banks was read. It gave a description of the fossil remains found in two fields near Brentford, Middlesex, in digging brick earth there. In the space of 120 square yards, nine teeth of the hippopotamus were found; also elephants' tusks nine feet long, deer's horns and teeth, river and marine shells, fish bones and teeth, &c. in great numbers.

bers. The strata were first gravel, then blue clay in which were numerous nodules, calcareous sand with shells, and clay with marine remains.

March 11. The Earl of Morton in the chair. A letter from Mr. Austin to Sir Humphry Davy, describing a new instrument made entirely of glass, for condensing gases in water, was read. Without drawings the description would not be intelligible.

March 18. Sir E. Home, Bart. through the Society for promoting Animal Chemistry, communicated the result of his observations and experiments for ascertaining the origin of animal fat and adipocire. He began by detailing his experiments on fowls, and particularly alluded to the cassowary of Java, which has a colon of only twelve inches, while that of Africa has one 45 feet long. This diversity he attributes to the wise œconomy of nature, the former country being extremely fertile, and the latter as sterile. This circumstance led him to examine the cause and effects of fat in the intestines, and the nourishment it affords to the entire animal; and hence he inferred that it is the intestines in all animals which supply the system with fat. Ambergrease, he observed, is the product of a disease, and is never found in whales above seven feet from the anus; it is usually from 14 lbs. to 100 lbs. and in one instance a piece weighing 182 lbs. had been found. Sir E. described the state of a woman buried in Shoreditch churchyard in 1790, ten feet below the surface of the ground, which is two feet below the level of a sewer passing through it, and leading to the Thames. In spring tides the sewer overflows, and the dead bodies are inundated. After eleven years, in 1801, the grave was opened, and the whole body was found to consist of adipocire. According to some experiments made by Mr. Brande, the animal fibre is converted into adipocire by immersion in gall; hence Sir E. concludes that the gall-bladder assists in accumulating fat, as well as other functions of the animal œconomy. Fat is rapidly formed, and as quickly absorbed; it soon accumulates in dormant animals, and is again absorbed during their sleeping season; it lies near the skin, and in old people supplies the place of muscle or fibre. The author related the case of a child born without any gall-bladder; it never became fat, nor yet wasted away, but died under a year old. Dr. Babington, in a letter to Sir Everard, mentioned a case where fat was voided like fæces; the patient had been ordered to take oil, and in consequence voided globules of fatty matter, which on examination were found to consist of

of one-third oil and two-thirds mucus. The author concludes that fat is not a secretion, as generally believed by physiologists.

March 25. In consequence of the death of Her Royal Highness the Duchess of Brunswick, the Society met only to adjourn till after the funeral of this princess.

GEOLOGICAL SOCIETY.

March 5. The Right Hon. the Marquis of Lansdowne;
The Right Hon. Charles Long, M. P.;
William Clarke, Esq. Trinity Coll. Cambridge,
were severally elected members of the Society.

Two letters from Mr. Webster, draughtsman and keeper of the museum to the Society, addressed to L. Horner, Esq. were read.

In the first Mr. W. states that during a late examination of the Isle of Wight, made by him for Sir H. Englefield, he discovered a series of calcareous strata of later formation than the chalk, and especially characterized by containing fresh-water shells. From this circumstance he was led to suspect a correspondence between this formation and the *calcaire d'eau douce*, which has been described by Brogniart and Cuvier as forming some of the strata in the basin of Paris; which conjecture was confirmed by a comparison of the fresh-water fossils of the Isle of Wight with those of the French strata which were given by M. Brogniart to the Count de Bournon, and by him have been deposited in the cabinet of the Geological Society.

In consequence of this interesting discovery, the attendance of Mr. Webster at the Society's apartments was for a time dispensed with, that he might re-examine the Isle of Wight and its vicinity. Accordingly his letter is dated from Freshwater, in the Isle of Wight, March 3d. In this he states that he has succeeded in obtaining some very desirable sections of the strata, and an abundant collection of specimens. He is inclined to think that there are two fresh-water formations, with a marine formation between. The lowest fresh-water formation consists of beds of sand and marle, with numerous fragments of the limnea of Lamarck, and of two species of planorbis; the interposed marine deposit is of blue clay, with venuses, oysters, and various turbinated shells: and the upper fresh-water formation consists of a calcareous rock inclosing numerous and very fine specimens of the limnea and planorbis. Some of this stratum is very friable, being only marle: other parts

parts are extremely hard, and the uppermost part of it has a porcelainous character. Mr. W. has not discovered any bed of gypsum, nor siliceous concretions.

A paper by Dr. MacCulloch, on the granite Tors of Devonshire and Cornwall, accompanied by three drawings, was read, and thanks were voted for the same.

The object of this communication is to show the process followed by nature in the destruction of the granite rocks of Cornwall and Devonshire, and to explain how this process is dependent upon a particular mode of aggregation of the materials of which this rock consists, and which can be satisfactorily observed only during the progress of its decomposition.

Dr. M. first treats of the granite which forms that promontory near the Land's End on which the Loggan rock is situated. Its length is about 200 yards, and the entire rock of which it is composed is traversed by numerous vertical and horizontal fissures, thus dividing the mass into a multitude of cubical and prismatic blocks. The Loggan rock itself, which is the subject of one of the drawings, appears to be one of these blocks in a state of decay, but still occupying its original site. Its general figure is irregularly prismatic and four-sided, with a protuberance on the lower surface on which it is poised. The breadth of the apparent surface of contact between this protuberance and the rock that it rests upon, is about a foot and a half; but its figure being cylindrical, and not spheroidal, the motion of the stone is limited to a vibration in one direction. The utmost force of three persons (according to Dr. M.'s trials) is only capable of making its exterior edge describe an arc the chord of which at six feet distance from the centre of motion is three quarters of an inch. A force of a very few pounds, however, is sufficient to begin and maintain a very visible degree of vibration: even the wind when blowing on its exposed western surface produces this effect very sensibly. Its weight, as estimated from its dimensions and the specific gravity of granite, appears to be 65 tons.

The subject of the second drawing is the Cheese-wring, which occupies the highest ridge of a hill to the north of Liskeard. It is an irregular column about 15 feet high, composed of five stones, the upper ones of which are so much larger than the rest as to overhang the base on all sides. The angles and external borders of these stones are considerably rounded by the effect of decomposition: and there is no doubt that in process of time this disintegration will proceed so far that the balance of the pile will be de-

 stroyed,

stroyed, and its ruins will not be distinguishabe from the other bowlders with which the tops of all the hills in this vicinity are overspread.

The third drawing represents the Vixen Tor on Dartmoor. Almost all the granite of Cornwall and Devon, like that of the Land's End, is divided by fissures into masses more or less approaching a cubical form. If a rock of this kind nearly level with the surface of the soil is examined, the fissures will be found to be a mere mathematical plane, and the angles formed by the intersection will be sharp and perfect. If we then turn our attention to granites which from their greater elevation above the soil appear to have been longer exposed to air and weather, we may observe a gentle rounding of the angles such as is exhibited in the Vixen Tor.

By degrees the fissures become wider; and the blocks, which were originally prismatic, acquire an irregular curvilinear boundary resembling those which form the Cheese-wring.

If the centre of gravity chances to be high, and far removed from the perpendicular of its fulcrum, the stone falls from its support, and becomes rounder by the progress of decomposition, till it assumes one of the various spheroidal figures which the granite bowlders so often exhibit.

These fissures, and the rounded form which the cubical blocks acquire by decomposition, Dr. M. is inclined to attribute to the original structure of the rock. In this, as in basalt, crystallization appears to have begun in distinct and more or less distant points, from each of which it proceeded forming thick concentric lamellæ, till at length the exterior shells of adjacent concretions came in contact, but were incapable of mutual penetration. The outer lamellæ are the least hard and dense, and therefore yield the easiest to the various causes which occasion the disintegration of rocks.

March 19. Mr. Webster exhibited specimens of the rocks containing fresh-water shells, recently discovered by him in the Isle of Wight.

A paper on the rocks at Clovelly in Devonshire, with illustrative drawings by the Rev. J. J. Conybeare, was read, and thanks were voted for the same.

The fishing town of Clovelly is situated in a narrow ravine, on the N. coast of Devon, about 22 miles W. of Ilfracombe. The shore is precipitous, being formed of cliffs about 130 or 140 feet in height, intersected by narrow alternations of grauwacke and grauwacke slate, curved and contorted in the most capricious way imaginable. No organic remains were observed in them, nor any foreign minerals,

minerals, except opaque white quartz forming numerous veins. Nearly the whole of north Devonshire is composed of the rock just described, which is locally distinguished into *dimstone* and *shillat*, the latter being the slaty grauwacke, and the former the compact. It is always very irregular in its stratification, is destitute of metallic veins, alternates with transition limestone, and, where it does so, occasionally contains organic remains: it also, in one instance at least, alternates with thick beds of a kind of culm; its veins, besides quartz, occasionally offer calcareous spar. Killas, which Mr. C. is inclined to regard as a variety, not of mica slate, but of clay slate, is contorted in its stratification only in the neighbourhood of the grauwacke, is traversed almost through its whole extent by frequent veins or dykes of a porphyritic rock which does not pass into the grauwacke, contains sometimes topaz, and not unfrequently garnet: its veins are often filled with chlorite mica and crystallized felspar, and also contain tin stone, gray cobalt ore, &c. These characters, in the opinion of the author of this paper, form a sufficient mark of distinction between the grauwacke and the killas of the West of England.

A paper on the island of Staffa, by Dr. MacCulloch, was read, and thanks were voted for the same.

The circumference of this island is about 2 miles; it forms a kind of table land of irregular surface, gently sloping to the N. E. and is bounded on all sides by steep and generally perpendicular cliffs, from 60 to 70 feet above high-water mark, the greatest elevation in the island being about 120 feet.

The entire island is a mass of basalt, but a bed of sandstone is said to be visible at low water on the western side.

The basalt presents two varieties, the columnar and amorphous, the latter of which is generally amygdaloidal, containing zeolites.

In the columnar variety lamellar stilbite is occasionally found filling the intervals of approximate columns, and sometimes, though rarely, in the substance of the smaller and more irregular columns.

On the south-western side of the island there appear to be three distinct beds of basalt, the lowermost of which seems to be amorphous. The next bed, from thirty to fifty feet thick, consists of those large columns which form the conspicuous feature of Staffa: the upper one appears, at a distance, to be a mass of amorphous basalt, but on closer inspection is found to consist of small columns, often

wood-laid and entangled in every direction. The columnar basalt of Staffa is by no means so regular as that of the Giant's Causeway; but, in return, it presents many beautiful specimens of bending columns, which do not occur at the latter place. Of these the most remarkable form a conical detached rock, called Budchaille or the Herdsman. Besides the great cave are two smaller ones, which being only accessible by a boat and in perfectly calm weather, can very rarely be examined.

The surface of the island is in many places overspread with a bed of alluvial matter, containing rounded fragments of granite, gneiss, mica slate, quartz, and red sand-stone, together with a few rolled pieces of basalt. As there is at present a considerable extent of deep sea between this bed and the nearest primitive rocks of Iona, Coll, Tirer and Mull, it becomes an interesting subject of speculation to inquire into the conditions requisite to account for this fact. These seem to be, either that a declivity sufficient to allow the transportation of rounded fragments has formerly existed sloping upwards from the present level of Staffa to some primitive mountains which no longer exist, or that the whole of Staffa itself has been raised to its present elevation from the bottom of the deep sea by which it is now surrounded.

MEDICAL SOCIETY OF LONDON.

The Anniversary of this Society was celebrated on the 8th of this month; when the following gentlemen were returned officers and members of the council for the year ensuing; viz.

J. C. Lettsom, M. and LL.D. &c. President.

Dr. Temple, Dr. Walshman, Mr. Norris, and Mr. Taunton, Vice Presidents.

Mr. Andree, Treasurer.

Dr. Hancock, Librarian.

Dr. Adams and Dr. Fothergill, Secretaries.

Dr. Taylor, Secretary for Foreign Correspondence.

Mr. T. Pettigrew, Registrar.

Council: Drs. Pinckard, Clutterbuck, Considen, Petch, Lidderdale.

Messrs. Jackson, B. Atkinson, E. Leese, Royston, A.T. Thomson, Blegborough, Bryant, Mathias, Hopkins, Abernethy, Brougham, Sawrey, Adams, Platt, Harvey, Bartlett, Combe, Harding, Sutleffe, Powell, Stevenson, W. K. Griffith, E. Austin, G. Young, Morgan.

To deliver the Anniversary Oration 1814, Dr. George Rees.

After

After the ballot, Mr. Saumarez delivered the Annual Oration on the Principles of Physiological and Physical Science, which was numerously attended; after which many of the Fellows and their friends adjourned to dinner.

We abstain, at present, from submitting an abstract of the oration, as we understand it is to be printed.

PHILOSOPHICAL SOCIETY OF LONDON.

The learned President, Dr. Lettsom, has during the last month delivered a lecture on the Natural and Medicinal Histories of Tea. He commenced by observing, that he could not be ignorant in presenting an object of discussion, that it is expected that some extent of novelty may be afforded, and some useful information conveyed; but from the universally known subject, he feared that disappointment must result; for what is familiar admits of little novelty, and what is known of little interest. "To convey instruction," said the Doctor, "I do not aspire; but impelled by a cordial impulse to evince my respect for and approbation of this Society, as well as excite the labours of others, I have presumed to throw the discus which a more youthful and vigorous arm is better qualified to direct." The lecturer then proceeded upon the botanical history, and, having given a description of the parts of fructification, stated, that there is but one species of the tea plant, the difference of green and bohea tea depending upon the nature of the soil, the culture, and manner of drying the leaves. Sir John Hill, from observing a different number of petals in different corollas, described the green and bohea tea as different species, giving to the first nine, and to the latter only six petals. He conveyed this opinion to Linné, who adopted the mistake, which his future experience corrected, as he informed Dr. L. by letter.

The authors who have treated upon this subject amount to at least a hundred, many of whom never saw the tea tree. The first figure given of it was in the Acta Haffniensia, which was taken from a dried specimen; since that it has been figured by Bontius, Plukenet, Kæmpfer, and Lettsom, the latter the only perfect one. He had access to the first plant grown in Europe, which was raised by his ingenious friend Mr. Ellis, in Gray's Inn, from seeds taken out of a canister, and promiscuously sown in a pot placed outside of his window. As China and Japan are the only countries known to us where the tea shrub is cultivated for use, we may reasonably conclude that it is indigenous to one of

them, if not to both, and probably the brackish ill-tasted water in many parts of those countries first led to its use as an infusion.

Tea was first introduced into Europe by the Dutch East India Company, early in the sixteenth century, and a quantity of it was brought over from Holland in 1666 by Lords Arlington and Ossory. In consequence of this, tea soon became known amongst people of fashion, and its use by degrees since that period has become general. Cornelius Boutekoe, a Dutch physician, wrote a treatise in praise of tea as early as 1678. The lecturer passed on to consider the soil and culture best adapted to this plant, and observed, that according to Kæmpfer, no particular gardens or fields are allotted for it, but that it is cultivated round the borders of rice and corn fields without any regard to the soil; that there are usually from six to twelve seeds in each vessel; that they are promiscuously put into a hole four or five inches deep at certain distances from each other. The reason why so many seeds are put into one hole is, that they contain a great quantity of oil, which is apt to turn rancid, and then they will not germinate. They then vegetate without further care. The leaves are not fit to be plucked before the third year's growth, and in about seven years the shrub rises to a man's height; but as it is then but scantily provided with leaves, it is cut down to the stem, from which an exuberance of fresh shoots arise. The tea tree delights particularly in valleys, or on the declivities of hill, and upon the banks of rivers where it enjoys a southern exposure to the sun; though it endures considerable variations of heat and cold, as it flourishes in the northern clime of Pekin, as well as about Canton.

The Doctor then proceeded to describe the manner and the seasons of gathering the leaves, and the method of curing or preparing tea in Japan. Of the varieties of tea, Dr. L. observed of the green, the bing, imperial, or bloom tea; the hy-tiann, hi-kiong, or hayssuen, known to us by the name of hyson, so called after an East India merchant of that name, who first imported it into Europe; and the singlo or songlo, which name it receives from the place where it is cultivated. Of the bohea teas, the soochuan or sutchong, by the Chinese called s-aaty-ang and saet-chaon or su-tyann; the camho or soumlo, called after the name of the place where it is gathered; the cong-fou, congo or bong-fo; the pekao, pecko, or pekoe, and the common bohea called moji by the Chinese.

The Doctor mentioned other kinds of tea which were rolled

rolled up in the form of balls and threads. He said he had formerly infused all the sorts of green and bohea teas he could procure, and expanded the different leaves on paper to compare their respective size and texture, intending thereby to discover their age. He found the leaves of green tea as large as those of bohea, and nearly as fibrous; which led him to suspect that the difference did not so much depend upon the age as upon the other circumstances.

The Asiatics give a flavour to tea by introducing among it the olea fragrans, whose small flowers are frequently to be met with in teas exported from China. On the subject of drinking of tea, Dr. L. observed, that the Chinese and Japanese never use tea before it has been kept a year, by which time its narcotic properties are diminished. They drink it without sugar or milk. Having mentioned various methods of preserving the seeds for vegetation, the lecturer entered upon its medical history.

It is natural to conclude, that as tea was imported from a foreign country, and at no inconsiderable danger and expense, and the custom of drinking it almost universal, much attention would have been excited respecting its natural and medical history, as well as its commercial influence; and indeed, as the learned President noticed, if saying much is a proof of attention, much has certainly been said and written, and much to no purpose, on its medicinal properties; for although he has examined nearly a hundred authors on the subject, he has acquired little information; nor can it be expected, where vague hypotheses are substituted for experiment, and theories for facts: thus claiming no fixed data, the inductions are fallacious or indecisive. This induced the Doctor to institute experiments, and establish principles upon which reason might exercise judgement, and truth elucidate facts. From these experments, which we are sorry our pages will not allow us room to relate, the sedative and relaxing effects of tea appear greatly to depend upon an odorous fragrant principle, which abounds most in green tea, particularly that which is most highly flavoured. This seems further confirmed by the practice of the Chinese, who avoid using this plant till it has been kept at least twelve months, as they find when recent it possesses a soporiferous and intoxicating quality.

The author deprecated the practice of taking tea very hot, and quoted in support of his opinion a passage from Professor Kalm's Travels into North America. The Doctor concluded by the following observations: " From the result of the experiments we may clearly explain the causes

of those different effects produced by tea-drinking; as well as upon what predominant qualities of this exotic these effects depend. Hence it will be inferred, that when the fine green teas are employed, whose sedative counterbalance their astringent qualities, and particularly in weak or delicate constitutions, debilitating and injurious effects may succeed; as tremors, fluttering and agitation of spirits, pain of the stomach, and weakened digestion, with flatulence, head-ache, and various nervous affections; and with such constitutions, this tea taken in the evening produces watching, and the unhappy sensations which want of the refreshment of sleep naturally produces: and may it not also be suspected, that the increased frequency of palsies and apoplexies may in some measure be attributed to the fragrant, odorous, and sedative influence of this exotic?"

"Indeed, from the whole analysis of green and bohea teas, the sedative and exhilarating qualities of the former will be clearly comprehended, as well as the astringent qualities of both; although, from the larger proportion of tannin in the bohea, it will be less relaxing; nevertheless combining such a proportion of odour as to give it a grateful influence on the nervous system; and thus, either single or mixed, they convey a pleasant and reviving sensation, as has been so often mentioned by travellers; and persons after fatigue of body, as well as exertion of mind, find in tea a grateful sedative and pleasing diluent."

KIRWANIAN SOCIETY OF DUBLIN.

Jan. 27. Mr. Carmichael concluded his paper "on the electric fluids, considered as different compounds of the solar rays." But as a long chain of induction, supported by references to a multiplicity of unconnected facts, and given in the most concise and naked form, scarcely admits of a perspicuous analysis, we are necessarily confined to a mere statement of the plan of the memoir.

It commences with the grounds for supposing that electricity is derived from the sun, as well as light and heat; and details the experiments made by M. Ritter, Dr. Wollaston, and Sir H. Davy, on the disoxygenating ray, whose effects so nearly resemble those of the Galvanic influence. After which are suggested such further experiments as would tend to a more satisfactory examination of the subject.

The possibility of chemical combinations, between the calorific, colorific, and disoxygenating rays, is next discussed: and it is supposed that the formation of one of the electric fluids is effected by a union between the disoxygenating

genating and calorific rays; and of the other, by a similar union between the disoxygenating and colorific.

Evidence is then adduced, to disprove the present supposition of the mutual repulsion of the homogeneous particles of electricity, and the mutual attraction of the heterogeneous; and the true laws that govern both fluids investigated. The laws proposed are as follow:

1. That in a free and unconfined state, at an indeterminate distance, there is a mutual attraction between any two volumes of electricity, whether of similar or different species.

2. That in the same free state, at a minute distance, all particles of electricity attract similar particles, and repel those of an opposite nature *.

3. That when contained in other bodies, an opposite effect takes place; and such bodies as are similarly electrified repel each other, and those which are differently electrified attract each other.

The phænomena of the Galvanic trough and Leyden phial are then explained, in reference to the proposed hypothesis; and such evidence as could be collected, is brought forward to prove that positive electricity is composed of the disoxygenating and colorific rays; and negative, of the disoxygenating and calorific. This is followed by a disquisition, whether it may not be either the colorific ray, or the calorific, and not the disoxygenating, which should be considered the base of both fluids. And the whole concludes with a consideration of the philosophical inquiries likely to arise out of the proposed hypothesis, if on due investigation it should be found worthy of adoption.

XXXVI. *Intelligence and Miscellaneous Articles.*

TRANSPARENT PAPER FOR ARTISTS.

THE tracing paper commonly used is apt to turn yellow, which injures its transparency and utility. The following recipe by Mr. Cathery, of Mead Row, near the Asylum, for a *white* transparent paper, appears in the xxxth volume of the Society for the Encouragement of Arts, &c. just published. The longer time this paper is made, the better it is; it keeps clear and white, and can be traced upon with a pen,

* It is necessary to state that the existence of the first position of this second law had occurred to Mr. Donovan, as well as to the author of the memoir, without any communication between them on the subject; and was enlarged on by him in a paper read to the Kirwanian Society before Mr. C. became a member.

a pen, if the ink has a little ox-gall put into it. Mr. Cathery sells it for the same price at which the common tracing paper is sold.

The Preparation.—Take one quart of the best rectified spirits of turpentine, and put to it a quarter of an ounce of sugar of lead finely powdered; shake it up, and let it stand a day and night; then pour it off, and add to it one pound of the best Canada balsam, set it in a gentle sand heat, and keep stirring it till it is quite mixed, when it will be fit for use; then lay your paper on a smooth board, and with a large brush, brush your paper over very even with the mixture, and then hang it upon lines to dry, and it will be fit for use in about four days.

The Society voted five guineas to Mr. Cathery for his communication.

EFFECTS OF COLOURED RAYS IN A MIXTURE OF OXY-MURIATE AND HYDROGEN GAS.

Mr. Leebeck, a German chemist, having made a mixture of these gases exposed them to the light of the sun, which suddenly decomposed them with a great explosion. This experiment was suggested by Gay Lussac and Thenard, and M. Leebeck has repeated it with success by means of gas collected over hot water. He afterwards introduced this gas into a yellowish red bell glass, and another of a deep blue, which he exposed to the solar rays. In the blue bell glass the decomposition took place instantly without any explosion, and in a minute at most it was ended, and the greater part of the bell glass was filled with water. On the contrary, in the red bell glass the decomposition took place very slowly: after being exposed for twenty minutes to very strong solar rays, a very small quantity of water rose in the red bell glass. This mixture of gas in the red bell glass was introduced into a white bell glass, and also exposed to the solar rays: no explosion took place, but in a few minutes the decomposition was complete, and the glass was filled with water. The experiments were several times repeated with similar results.

LIST OF PATENTS FOR NEW INVENTIONS.

To William Chapman, of Murton House, in the county of Durham, civil engineer, and Edward Walton Chapman, of Wellington Ropery, in the parish of Walls End, in the county of Northumberland, rope-maker, for their methods of facilitating the means and reducing the expense of carriage on rail ways and other roads.—30th December, 1812.

To

To Joseph Raynor, of Sheffield, in the county of York, cotton spinner, for his improved machinery for roving and spinning cotton, silk, flax, and wool.—1st January, 1813.

To William Wilkinson, of Grimesthorpe, in the county of York, shear-smith, for his horse shears, wool shears and glovers' shears.—5th January.

To Thomas Ryland, of Birmingham, in the county of Warwick, plater, for his fender for fire places.—15th Jan.

To John Shorter Morris, of North Market Street, Kennington, in the county of Surry, mechanic, for his machine or engine upon a new and superior principle, which contains a new way for a man or men to use his or their power and strength, to be used as a crane, or to give a rotary motion to any machine, engine, or mill-work.—15th Jan.

To Robert Dickinson, of Great Queen Street, Lincoln's Inn Fields, in the county of Middlesex, esq. for his improvement in vessels for containing liquids.—15th Jan.

To William Bundy, of Camden Town, in the county of Middlesex, mathematical instrument maker, for his new manufacture of lint.—15th Jan.

To Matthew Bush, of Longford, in the county of Middlesex, calico printer, for his improvements for printing calicoes.—15th Jan.

To William Allen, of the Curtain Road, Shoreditch, for his improvement on machinery to be worked by wind.—15th Jan.

To Richard Cawkwell, of Newark upon Trent, in the county of Nottingham, miller, for his improved machine for washing, cleansing, and scouring linen and woollen goods and other articles.—15th Jan.

To Charles Gröll, of Leicester Place, Leicester Square, in the county of Middlesex, and Frederick Dizi, of Park Place, Baker Street North, in the said county of Middlesex, for certain improvements on harps.—22d Jan.

To Marc Isambard Brunel, of Chelsea, in the county of Middlesex, civil engineer, for certain improvements in saw-mills.—20th Jan.

To Francis Crow, of Feversham, in the county of Kent, watchmaker and silversmith, for certain improvements in the mariner's compass or boat compass.—30th Jan.

To Robert Dunkin, of the Town of Penzance, in the county of Cornwall, for his methods for lessening the consumption of steam and fuel in working fire-engines, and also methods for the improvement of certain instruments useful for mining or other purposes.—30th Jan.

To George Alexander, watchmaker, in Leith, for his improved

proved mode of suspending the card of the mariner's compass, being on a principle entirely new.—4th Feb.

To William Broughton, of Rose Court, Tower Street, in the city of London, joiner, for a method of making a peculiar species of canvass which may be used more advantageously for military and other purposes than the canvass now in use.—4th Feb.

To Peter Ewart, of Manchester, in the county of Lancaster, cotton manufacturer, for his method of working weaving looms by machinery.—20th Feb.

To Joseph Hamilton, of the city of Dublin, gentleman, for certain new methods of constructing and connecting earthen building materials.—20th Feb.

To Charles Plimley, of Birmingham, in the county of Warwick, manufacturer, for his methods of working steel or iron, or steel joined with iron, in or into taper forms whether round or square, or of any other figure in the cross sections thereof, for the purpose of making files and various other articles.—20th Feb.

To John Roberts, of Macclesfield, in the county of Chester, cotton spinner, for a method of contracting or reducing into small compass such part of malt and hops as are requisite in making ale, beer, and porter.—20th Feb.

To Joseph Smith, of Coseley, in the parish of Sedgley, in the county of Stafford, iron and coal master, for certain improvements in the construction and manufacture of iron and other chains, whereby a considerable expense will be saved in the making thereof, and the same rendered more durable.—24th Feb.

To John White, of Princes Street, Soho, for his machine for cooking without wood or coal.—3d March.

To James Thomson, of Primrose Hill, near Clithero, in the county of Lancaster, calico-printer, for his method of producing patterns on cloth previously dyed Turkey red, and made of cotton, or linen, or both.—3d March.

To Alexis Delahante, of Great Marlborough Street, that in consequence of a communication made to him by a learned foreigner residing in parts abroad, he is possessed of a method for the production or the making of a green colour, and the application thereof to various useful purposes.—3d March.

To Richard Green, of Lisle Street, Leicester Square, in the county of Middlesex, sadlers' ironmonger, for his stirrup with a spring in the eye, and a spring bottom, for the safety of persons riding on horseback, and to prevent their being dragged in the stirrup.—3d March.

To

To Sir Thomas Cochrane, commonly called Lord Cochrane, for his method or methods of more completely lighting cities, towns, and villages.—3d March.

Meteorological Observations made at Cambridge from February 12 *to March* 17, 1813.

Feb. 12.—Cloudy with small rain, and wind from SW.

Feb. 13 —Fair morning at times with showers, and *cirrus, cirrocumulus,* &c. in the intervals as usual; fine moonlight evening, with *cirrocumulus;* later in the night the moon was hazy and had a *halo* round her, followed by rain next day as usual.

Feb. 14.—Rain and wind from SW all day.

Feb. 15.—Showery and windy, and those appearances of the clouds which attend cloudy weather. Clear night.

Feb. 16.—Clear windy day; various modifications.

Feb. 17.—Clear, and clouds of different modifications, and showers. Thermometer 11 P.M. 46.

Feb. 18.—Clear morning; afterwards some clouds; clear, and clouds again at night. Wind westerly. Thermometer 11 P.M. 42.

Feb. 19.—Windy; various clouds. SW.

Feb. 20.—Windy; *cirrus,* and other clouds; clouded over at intervals; fine starlight night. Thermometer at 3 P.M. 53, at 11, 46. Wind SW.

Feb. 21.—Overcast with some occasional small rain. Thermometer at 4 P.M. 55, and the same at 11 at night. Wind SW.

Feb. 22.—Various features of *cirrus* of the rainy character, with heavier masses of clouds lower, and scud much below, floating in the wind; at night some rain. Thermometer down to 49° at 11 P.M. Wind SW.

Feb. 23.—Overcast early, with small rain; afterwards fair, with rainy features of *cirrus* * spread about above other clouds.

Feb. 24.—Parhelion observed early near Cambridge, *cirrus* and others; showers of hail at times.

Feb. 25.—White frost followed by some rain. Therm. at 7 A.M. 36°, at 3 P.M. 48°, at 11 P.M. 47°.

Feb. 26.—Thermometer 7 A.M. 48°, 2 P.M. 52°, 11 P.M. 45°; clouded for the most part, with some rain at night. Wind SW.

* By rainy features is meant, the confused and mistlike kind of *cirrus* which accompanies rainy weather, and is contrasted to the fibrous and angular appearance of this cloud when the air is dry.

Feb.

Feb. 28.—Fine clear morning; a few *cirri;* through the day only *cumuli* appeared; towards the evening a few *cirri* again of transitory and indefinite kind; clear night. Thermometer 49° 11 A.M., 35° 11 P.M.; a westerly gale.

March 1.—Some *cirri* and *cumuli,* with occasional *cumulostratus,* with a gale; overcast night. SW. Therm. 51° at 2 P.M., at 11 48°.

March 2.—Showery; with *cumuli,* and scud below, and *cirrus* confused and transitory in the clear intervals. Thermometer, middle of the day, 50°, 11 P.M. 35°. W. Some falling stars of the common kind *.

March 3.—Showers, *cirrus, cumulostratus,* scud, &c. in the intervals like yesterday. Thermometer 11 P.M. 37°. Westerly wind.

March 4.—Chiefly cloudy, dull day. Wind westerly.

March 5.—Early among various clouds I observed the *cirrocumulus,* whose *nubeculæ* were flimsy and ill-defined. In a low region *cumuli* flew along of an ill-defined kind. Gale from SW. in evening. Thermometer 2 P.M. 55°, at 11, 44.

March 6.—Clear early; I afterwards observed *cirri* in fibres flimsy and of short duration, but through the chief part of the day only *cumuli* more or less dense appeared. Wind strong from the west. Therm. 2, 53°, at 11, 40°. Clear night.

March 7.—Such irregular features of *cirrocumulus* and *cirrus* above *cumuli, cumulostratus,* and scud, as usually attend showery weather, but no rain fell. Clouded over at night. Therm. at 7 A.M. 45°, 3 P.M. 55°, 11 P.M. 44. Wind westerly.

March 8.—*Cumuli* in abundance, fleecy and lowering; sun at intervals; dark clouded night. Wind SW. Therm. 7 A.M. 49°, 2 P.M. 57°.

March 9.—Chiefly cloudy, dark night with rain. Therm. 44° at 11 P.M. Wind SW.

March 10.—Cold wet mist in the morning; afterwards it cleared by time, when smokelike *cumuli,* &c. abounded; a hard shower about one P.M.; cloudy afternoon, and cold night with lucid intervals; some flimsy large ill-defined *cirrocumulus* †. Therm. 7 A.M. 39°, 2 P.M. 44°, 11 P.M. 33°. Wind NE.

* See observations on the peculiarities of meteors, in my letter in your Magazine for November 1811.

† I suspect that if an electrophorus was tried, the air today would be found either non-electric or electrified negatively. If such experiment should have been made, I should be obliged by its communication in your Magazine.

March

March 11.—The thermometer has been down below the freezing point in the night. Snow is now falling (8 A.M.) when there is no dense cloud near the zenith, only some of the fleecy kind; there is however a denser kind of cloud in the horizon; afterwards common snow showers prevailed, with clear intervals, and strong wind from NE. Therm. at 11 P.M. as low as 29°; clear star-light night, at intervals with wind.

March 12.—Snow showers, in which the *nimbi* never appeared very dense; the same phænomenon of snow falling, when there was no *nimbus* near the zenith, happened again today. Either the snow must have been blown along horizontally for some distance by a strong wind, or must have come from light clouds not suspected to be *nimbi* from spectators below. These clouds were a kind of confused *cirrus* of light texture. The night was clear, and Therm. at 11, 29°.

March 13.—Cloudy, and sun at times; by night large and elevated masses of cloud of no great density covered the sky, through which the moon appeared, with a *corona* at times. Therm. at 11 P.M. 33°. Barometer 30° 20″. Wind NE.

March 14.—Cold, cloudy, damp day, with some rain, and moderate wind, which got to the W. in afternoon. Therm. 7 A.M. 41°, 11 P M. also 41°. The moon appeared at times through the thinner parts of the clouds.

March 15.—Warmer and overcast sky. Therm. at noon 54°; some rain came on in the evening, and the Thermometer was 49°. Wind southerly.

March 16.—Warmer weather than yesterday. Therm. at 3 P.M. 54°, at 11 P.M. 48°. Cloudy with small rain all day, but it held up at night. Wind southerly.

March 17.—Fine clear warm day, only a few flimsy *cirri* aloft. Therm. 3 P.M. 56°; in the evening it became cooler, the moon got hazy with a faint small *corona* round her, a large elevated arc of confused *cirrus* appeared a long time stationary in the north, and at half after ten o'clock a faint lunar *halo* began to be discoverable; the Thermometer being 38°. Wind easterly.

N. B.—The observations for the 12th and 13th of February, were made *not* at Cambridge, but in Essex, about twenty miles S. of Cambridge.

Corpus Christi College, Cambridge,
March 18, 1813.

THOMAS FORSTER.

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For March 1813.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Feb. 25	32	47	47	30·08	24	Cloudy
26	47	52	46	29·78	0	Rain
27	36	51	38	30·20	36	Fair
28	34	50	36	·40	40	Fair
March 1	36	47	47	·20	36	Fair
2	47	52	36	·19	30	Cloudy
3	36	47	38	·23	37	Fair
4	40	51	36	·30	42	Fair
5	34	48	38	·32	45	Fair
6	36	51	37	·38	42	Fair
7	35	47	45	·42	46	Fair
8	44	54	46	·25	44	Fair
9	40	47	40	·05	38	Fair
10	36	43	35	29·95	0	Rain
11	34	38	32	30·02	0	Sleet
12	26	36	28	·28	26	Fair
13	27	38	33	·31	25	Fair
14	33	42	42	·20	15	Showery
15	42	47	45	·21	17	Cloudy
16	45	43	36	·20	15	Showery
17	35	52	41	29·98	29	Fair
18	35	54	43	·92	36	Fair
19	42	56	44	·86	38	Cloudy
20	43	57	44	·76	38	Cloudy
21	42	54	42	·92	42	Cloudy
22	44	54	36	·75	37	Showery
23	33	40	37	30·30	36	Fair
24	40	47	39	·19	0	Rain
25	40	47	41	·19	0	Rain
26	40	45	39	·43	32	Cloudy

N.B. The Barometer's height is taken at one o'clock.

XXXVII. *An Investigation of the Properties of Lactic Acid.* By JACOB BERZELIUS, *Professor of Medicine and Pharmacy, and M.R.A. Stockholm. Translated from the Swedish*.*

THE lactic acid was discovered by Scheele. He allowed sour milk to coagulate, filtered the fluid, and boiled it away to 1 8th; he then filtered it again, to separate the curd which was deposited. This acid fluid he saturated with lime water, which threw down a precipitate of bone earth; the remaining lime was cautiously separated by the addition of oxalic acid, taking care that it should not be in excess, and the fluid was evaporated to dryness. The acid was then dissolved in alcohol, which, after evaporation, afforded it in the purest state that was then known. Scheele determined the relations of this acid to the greatest number of bases so clearly and accurately, that there was no reason to entertain any doubt of its proper nature, as essentially distinct from all other acids. The assertions of Scheele have however within the last few years not only been called in question, but even positively contradicted, by some French chemists, Bouillon Lagrange, Thenard, Fourcroy, and Vauquelin. Since only the two last chemists have had sufficient confidence in their scientific character to decide positively on this question, in contradiction to the results of Scheele's experiments, I shall here only say a few words respecting their assertions. Among the properties of the lactic acid, Scheele has mentioned that it is not volatile, and that it is destroyed by distillation, and affords a sour spirit; a name, by which the empyreumatic vinegar, which is obtained in the distillation of organic substances, was then always distinguished. In their investigations respecting this acid, the French chemists saturated it with an alkali, mixed the salt with concentrated sulphuric acid, and distilled the mixture. The liquid distilled contained a weak vinegar, which was saturated with alkali, was evaporated to dryness, and was distilled again with sulphuric acid. It requires no great depth of chemical knowledge to discover the impropriety of this mode of operation, and the necessity of the formation of vinegar at the expense of the component parts of the lactic acid destroyed. On the other hand, these chemists seem not to have paid the slightest attention to the proper characteristic salts, which are produced by the lactic acid with different bases, and from which

* From the Animal Chemistry of Professor Berzelius, vol. ii. p. 430.

which we usually derive the principal and the most certain criterions of an acid. They concluded from their experiments, that Scheele had been mistaken, and that the lactic acid was nothing else than a combination of the acetic with a proper animal matter, which hindered the volatilisation of the acid.

Since we have found that the lactic acid acts a more or less distinguished part in the blood, in muscular flesh, in the marrow, in urine, and in milk, it appears desirable that these circumstances should be very accurately examined. I have therefore undertaken to investigate its nature very fully, and have obtained, by means of a long and often repeated series of different experiments, a complete conviction that Scheele was in the right, and that the lactic acid is a peculiar acid, very distinct from all others.

I shall here relate the results of my experiments. The extract which is obtained when dried whey is digested with alcohol, contains, as I have already observed, uncombined lactic acid, lactate of potass, muriate of potass, and a proper animal matter. I mixed this solution in alcohol with another portion of alcohol to which $\frac{1}{75}$ of concentrated sulphuric acid had been added, and continued to add fresh portions of this mixture as long as any saline precipitate was formed, and until the fluid had acquired a decidedly acid taste. Some sulphate of potass was precipitated, and there remained in the alcohol muriatic acid, lactic acid, sulphuric acid, and a minute portion of phosphoric acid, detached from some bone earth which had been held in solution. The acid liquor was filtered, and afterwards digested with carbonate of lead, which with the lactic acid affords a salt soluble in alcohol. As soon as the mixture had acquired a sweetish taste, the three mineral acids had fallen down in combination with the lead, and the lactic acid remained behind, imperfectly saturated by a portion of it, from which it was detached by means of sulphuretted hydrogen, and then evaporated to the consistence of a thick varnish, of a dark-brown colour and sharp acid taste, but altogether without smell.

In order to free it from the animal matter which might remain combined with it, I boiled it with a mixture of a large quantity of fresh lime and water, so that the animal substances were precipitated and destroyed by the lime. The lime became yellow brown, and the solution almost colourless, while the mass emitted a smell of soap lees, which disappeared as the boiling was continued. The fluid thus obtained was filtered and evaporated, until a great part of

of the superfluous lime held in solution was precipitated. A small portion of it was then decomposed by oxalic acid, and carbonate of silver was dissolved in the uncombined lactic acid, until it was fully saturated. With the assistance of the lactate of silver thus obtained, a further quantity of muriatic acid was separated from the lactate of lime, which was then decomposed by pure oxalic acid, free from nitric acid, taking care to leave it in such a state that neither the oxalic acid nor lime water afforded a precipitate. It was then evaporated to dryness, and dissolved again in alcohol, a small portion of oxalate of lime, before retained in union with the acid, now remaining undissolved. The alcohol was evaporated until the mass was no longer fluid while warm; it became a brown clear transparent acid, which was the lactic acid, free from all substances that we have hitherto had reason to think likely to contaminate it.

The lactic acid, thus purified, has a brown yellow colour, and a sharp sour taste, which is much weakened by diluting it with water. It is without smell in the cold, but emits, when heated, a sharp sour smell, not unlike that of sublimed oxalic acid. It cannot be made to crystallize, and does not exhibit the slightest appearance of a saline substance, but dries into a thick and smooth varnish, which slowly attracts moisture from the air. It is very easily soluble in alcohol. Heated in a gold spoon over the flame of a candle, it first boils, and then its pungent acid smell becomes very manifest, but extremely distinct from that of the acetic acid; afterwards it is charred, and has an empyreumatic, but by no means an animal smell. A porous charcoal is left behind, which does not readily burn to ashes. When distilled, it gives an empyreumatic oil, water, empyreumatic vinegar, carbonic acid, and inflammable gases. With alkalis, earths, and metallic oxides, it affords peculiar salts; and these are distinguished by being soluble in alcohol, and in general by not having the least disposition to crystallize, but drying into a mass like gum, which slowly becomes moist in the air.

Lactate of potass is obtained, when the lactate of lime, purified as has been mentioned, is mixed warm with a warm solution of carbonate of potass. It forms in drying a gummy, light yellow brown, transparent mass, which cannot easily be made hard. If it is mixed with concentrated sulphuric acid, no smell of acetic acid is perceived; but if the mixture is heated, it acquires a disagreeable pungent smell, which is observable in all animal substances mixed

 with

with the sulphuric acid. The extract, which is obtained directly from milk, contains this salt; but this affords, when mixed with sulphuric acid, a sharp acid smell, not unlike that of the acetic acid. This however depends not on acetic but on muriatic acid, which in its concentrated state introduces this modification into the smell of almost all organic bodies. The pure lactate of potass is easily soluble in alcohol; that which contains an excess of potass, or is still contaminated with the animal matter soluble in alcohol, which is destroyed by the treatment with lime, is slowly soluble, and requires about 14 parts of warm alcohol for its solution. It is dissolved in boiling alcohol more abundantly than in cold, and separates from it, while it is cooling, in the form of hard drops.

The *lactate of soda* resembles that of potass, and can only be distinguished from it by analysis.

Lactate of ammonia. If concentrated lactic acid is saturated with caustic ammonia in excess, the mixture acquires a strong volatile smell, not unlike that of the acetate or formiate of ammonia, which however soon ceases. The salt which is left has sometimes a slight tendency to shoot into crystals. It affords a gummy mass, which in the air acquires an excess of acidity. When heated, a great part of the alkali is expelled, and a very acid salt remains, which deliquesces in the air.

The *lactate of baryta* may be obtained in the same way as that of lime; but it then contains an excess of the base. When evaporated, it affords a gummy mass, soluble in alcohol. A portion remains undissolved, which is a subsalt, is doughy, and has a browner colour. That which is dissolved in the alcohol affords by evaporation an almost colourless gummy mass, which hardens into a stiff but not a brittle varnish. It does not show the least tendency to crystallize. The salt which is less soluble in alcohol may be further purified from the animal matter adhering to it, by adding to it more baryta, and then becomes more soluble.

The *lactate of lime* is obtained in the manner above described. It affords a gummy mass, which is also divided by alcohol into two portions. The larger portion is soluble, and gives a shining varnish inclining to a light yellow colour, which, when slowly dried, cracks all over, and becomes opake. This is pure lactate of lime. That which is insoluble in alcohol is a powder, with excess of the base; received on a filter, it becomes smooth in the air like gum,

or

or like malate of lime. By boiling with more lime, and by the precipitation of the superfluous base upon exposure to the air, it becomes pure and soluble in alcohol.

Lactate of magnesia, evaporated to the consistence of a thin syrup, and left in a warm place, shoots into small granular crystals. When hastily evaporated to dryness, it affords a gummy mass. With regard to alcohol, its properties resemble those of the two preceding salts.

Ammoniaco-magnesian lactate is obtained by mixing the preceding salt with caustic ammonia, as long as any precipitation continues. By spontaneous evaporation this salt shoots into needle-shaped prisms, which are little coloured, and do not change in the air. I have once seen these crystals form in the alcoholic extract of milk boiled to dryness: but this is by no means a common occurrence.

The *lactate of silver* is procured by dissolving the carbonate in the lactic acid. The solution is of a light yellow somewhat inclining to green, and has an unpleasant taste of verdigris. When evaporated in a flat vessel, it dries into a very transparent greenish yellow varnish, which has externally an unusual splendour like that of a looking-glass. If the evaporation is conducted in a deeper vessel, and with a stronger heat, a part of the salt is decomposed, and remains brown from the reduction of the silver. If this salt is dissolved in water, no inconsiderable portion of the silver is reduced and deposited, even when the salt has been transparent; and the concentrated solution has a fine greenish yellow colour, which by dilution becomes yellow. If we dissolve the oxide of silver in an impure acid, the salt becomes brown, and more silver is revived during the evaporation.

The *lactate of the protoxide of mercury* is obtained when the lactic acid is saturated with black oxidated mercury. It has a light yellow colour, which disappears by means of repeated solution and evaporation. The salt exhibits acid properties, deliquesces in the air, and is partially dissolved in alcohol, but is at the same time decomposed, and deposits carbonate of mercury, while the mixture acquires a slight smell of ether. The lactic acid dissolves also the red oxide of mercury, and gives with it a red gummy deliquescent salt. If it is left exposed to a warm and moist atmosphere, it deposits, after the expiration of some weeks, a light semi-crystalline powder, which I have not examined, but which probably must be acetate of mercury.

The *lactate of lead* may be obtained in several different

degrees

degrees of saturation. If the lactic acid is digested with the carbonate of lead, it becomes browner than before, but cannot be fully saturated with the oxide; and we obtain an acid salt, which does not crystallize, but dries into a syrup-like brown mass, with a sweet austere taste. When a solution of lactic acid in alcohol is digested with finely powdered litharge, until the solution becomes sweet, and is then slowly evaporated to the consistence of honey, the neutral lactate of lead crystallizes in small grayish grains, which may be rinsed with alcohol, to wash off the viscid mass that adheres to them, and will then appear as a gray granular salt, which when dry is light and silvery, like the precipitate thrown down by alcohol from a precipitated alkali. It is not changed in the air; treated with sulphuretted hydrogen, it affords pure lactic acid. If the lactic acid is digested with a greater portion of levigated litharge than is required for its saturation, the fluid acquires first a browner colour, and as the digestion is continued, the colour becomes more and more pale, and the oxide swells into a bulky powder, of a colour somewhat lighter than before. If the fluid is evaporated, and water is then poured on the dry mass, a very small portion of it only is dissolved; the solution is not coloured, and when it is exposed to the air, a pellicle of carbonate of lead is separated from it. If the dried salt of lead be boiled with water, and the solution be filtered while hot, a great part of that which had been dissolved will be precipitated while it cools, in the form of a white or light yellow powder, which is a sublactate of lead. This salt is of a light flame colour; when dried, it remains mealy, and soft to the touch, and it is decomposed by the weakest acids, while the acid salt is dissolved in water, exhibiting a sweet taste and a brown colour. When moistened with water, it undergoes this change from the operation of the carbonic acid diffused in the air. If this salt is warmed and then set on fire at one point, it burns like tinder, and leaves the lead in great measure reduced. A hundred parts of this salt, dissolved in nitric acid, and precipitated with carbonate of potass, gave exactly 100 parts of carbonate of lead; consequently its component parts, determined from those of the carbonate, must be 83 of the oxide of lead, and 17 of the lactic acid. At the same time we cannot wholly depend on this proportion, and it certainly makes the quantity of lead somewhat too great. The relation of the lactic acid to lead affords one of the best methods of recognising it, and I have always principally employed

employed it, in extracting this acid from animal fluids; it gives the clearest distinction between the lactic acid and the acetic.

The *lactate of iron* is of a red brown colour, does not crystallize, and is not soluble in alcohol. The *lactate of zinc* crystallizes. Both these metals are dissolved by the lactic acid, with an extrication of hydrogen gas. The *lactate of copper*, according to its different degrees of saturation, varies from blue to green and dark blue. It does not crystallize.

It is only necessary to compare the descriptions of these salts with what we know of the salts which are formed with the same bases by other acids, for example, the acetic, the malic, and others, in order to be completely convinced that the lactic acid must be a peculiar acid, perfectly distinct from all others.

XXXVIII. *Critical Observations on Dr.* WOLLASTON'S *stated Improvement of the Camera Obscura and Microscope in the Application of the Meniscus and two Plano-convex Lenses; proving their Inferiority to the double Convex Lens generally used. By* WILLIAM JONES, *Optician.*

To Mr. Tilloch.

SIR,—IN your impartial Journal, vol. xvii. and also in another cotemporary Journal *, some observations of mine were published, proving satisfactorily, I trust, that the periscopic spectacle glass advertised by Dr. Wollaston as possessing a new optical principle, and affording an improvement in the figure of a spectacle glass, was no other than the old rejected meniscus lens; contained no refractive property different from the plano-convex and double convex lenses; but, as it caused a greater degree of aberration than those two lenses, was a worse form of lens for spectacles or any other instrument than the double convex lens generally used by practical opticians;—it must, therefore, surprise others conversant in optics, besides myself, that Dr. Wollaston should be induced again to propose the meniscus in the camera obscura instead of the double convex lens; his account of which is copied into your journal of last month from the Philosophical Transactions for 1812.

The desire that I have to maintain an optical truth, and the duty I owe to our professional interests, oblige me to

* Nicholson's Journal, vol. vii.

point out to your readers, what I consider to be the error of his reasoning, and the fallacy of his inference.

In his description of the effect of the double convex lens in the common camera, page 90, he states the known effect of the images distant from the middle, or direct focus of the lens, being somewhat indistinct, on account of the plane of representation becoming, in distance, greater than the principal focus of the lens, and the oblique pencils of rays being refracted to a focus rather shorter than the principal one. "On this account (he adds) it is in general best to place the lens at a distance somewhat less than that which would give most distinctness to the central images, because, in that case, a certain moderate extension is given to the field of view, from an adjustment better adapted to lateral objects, without materially impairing the brightness of those in the centre." The aberrations of the lens also add to the indistinctness.

The collateral indistinctness in our portable chest cameras is but trivial and unimportant; and, in my opinion, the remedy, as above proposed, will be found by the artist to be worse than the defect, as the distinct and vivid central images will be vitiated, and the extreme images but very little improved. The most perfect remedy is that which has been used by opticians in large cameras, for more than fifty years, of placing a bottom board, or whitered table, with a concave surface, proportioned to the focal distance of the lens; which, corresponding very nearly to the focus of all oblique refracted rays, exhibits universally the images with the greatest brilliancy and distinctness. The exact curve of the surface of this board or table should be that of a conic section; but the concave one answers sufficiently well. It is necessary for the reader unskilled in optics to know, that what opticians name the axis of a lens, is that imaginary line that is supposed to pass through its centre, is not subject to any refraction, and all other rays incident on its surface are refrangible, in proportion to the angle they make with this axis; those rays impinging nearest the centre of the lens, and, with the least obliquity of position, are refracted with the most perfect images, or with the least aberration, in double convex, plano-convex, and meniscus lenses. The longitudinal aberration produces a focus short of the principal one, and the lateral aberration a confused lateral extension of the images blended with prismatic colour. These aberrations increase directly with the diameter and thickness of the lens, and inversely with its focus. In lenses of large diameter and short foci these aberrations will,

will, by experiment, be rendered very manifest, and which have been clearly demonstrated by that learned optician Mr. Benjamin Martin, in his Elements of Optics, by Dr. Smith, and by others.

The subsequent paragraph, at page 91, describes Dr. Wollaston's proposed improvement: the substance, in his own words, is as follows:

"The lens is a meniscus, with the curvatures of its surfaces about in the proportion of two to one, so placed that its concavity is presented to the objects, and its convexity towards the plane on which the images are formed. The aperture of the lens is four inches, its focus about twenty-two. There is also a circular opening, two inches in diameter, placed at about one-eighth of the focal length of the lens from its concave side, as the means of determining the quantity and direction of rays that are to be transmitted.

"The advantage of this construction over the common camera obscura is such, that no one who makes the comparison can doubt of its superiority; but the causes of this may require some explanation. It has been already observed, that by the common lens, any oblique pencil of rays is brought to a focus at a distance less than that of the principal focus. But in the construction above described, the focal distance of oblique pencils is not merely as great, but is greater than that of a direct pencil. For since the effect of the first surface is to occasion divergence of parallel rays, and thereby to elongate the focus ultimately produced by the second surface, and since the degree of that divergence is increased by obliquity of incidence, the focal length resulting from the combined action of both surfaces will be greater than in the centre, if the incidence on the second surface be not so oblique as to increase the convergence. On this account, the opening E is placed so much nearer to the lens than the centre of its second surface, that oblique rays E*f*, after being refracted at the first surface, are transmitted through the lens nearly in the direction of its shorter radius, and hence are made to converge to a point so distant that the image (at *f*) falls very nearly in the same plane with that of an object centrally placed."

The radii of curvatures for a meniscus of twenty-two inches focus, being as two to one, is not essential. The theory of Dioptrics shows that the greater the proportion, or the nearer that the radius of one side approaches to infinity or the side to a plano, the more perfect the lens will be. Dr. Wollaston has not stated the diameter of the convex lens, but the reader must suppose it to be four inches, like

like that of the meniscus; nor has he told the reader what improvement would be produced, if he placed a similar circular opening, or limited aperture, also over the convex lens. I must, therefore, inform the reader; and he may himself prove it to be correct. The diameter or aperture of four inches is too great for a lens of twenty-two inches focus, either double convex or meniscus lens, placed in a camera obscura; as it transmits too much light, and produces too much aberration for the most distinct representation of the images within the camera. Dr. Wollaston therefore, no doubt, was obliged to correct this palpable defect, by a curtailment of the area of his lens no less than *three-fourths* of the whole, and the lens would have been more like one applied by a skilful optician, if he had at first inserted a lens of about two inches diameter. The limited aperture therefore, it is evident, advantageously excludes rays, but has nothing to do with the *determination* of their *direction*. Upon a fair comparison, the reader will not only doubt of the superiority of Dr. Wollaston's camera, but be *convinced* of its absolute inferiority; for the double convex lens, under the same diameter and focus as the meniscus, has less spherical surface, and consequently less longitudinal and lateral aberration of the two. Let us now advert to the transformation of the double convex lens to become a meniscus with the same focus: by considering their figures in his diagrams, the reader will perceive, that as much as the upper surface of the convex has been incurvated for a meniscus, so much the more has the convexity of the under side been augmented, to retain the original focus. The oblique pencils of rays first entering the meniscus, or any part of its surface, are, from the immutable laws of refraction, refracted from the axis of the lens, contrariwise, to the first direction of the convex, and, afterwards in their passage into air, by the increased inferior convexity, refracted back towards the axis proportionally more than by the under side of the double convex to be converged to the same focal distance; and all pencils of rays that impinge on the surface in an oblique direction to its axis, must be united the same as by the convex lens, at a focus somewhat shorter than the principal focus from direct rays. The meniscus lens, in refractive property, differs not from the double convex one. The above explanation is agreeable to all writers on optics, and to correct experiment. In this meniscus it is not "*if* the incidence," &c. but, the incidence *always* is so oblique on the second surface as to increase the convergence; and no kind of

of opening, E, whatever will change Nature's laws of refraction so as to elongate the focus, or to produce two different focuses in one lens; and his previous explanation of "occasioning all pencils to pass as nearly as may be at right angles to the surfaces of the lens," page 90, is an irrelevancy in optics, and is the error of reasoning I formerly imputed to Dr. Wollaston in his spectacle glass. It is the angle that the rays make with the *axis of the lens*, of whatever shape, that refraction is estimated from, as the science teaches us; but not from the geometrical positions of pencils and surfaces. From the greater aberration that the meniscus possesses, the images formed by it will be less distinct, have less light, and be more distorted than by the double convex lens. It is from the extended lateral distortion, and bringing the meniscus nearer to the plane than its exact focus, that I can assign a cause how Dr. Wollaston could have fallen into the error: had he placed the concave side downwards, it would have been a better position, the images would have been more defined and enlightened; it was so applied in his spectacles, the convex side being next to the object; but in neither case will the images be so perfect and vivid as by the double convex lens. The meniscus in a camera is not a new application; several, some years back, were made, but not preferred. I can refer to a machine now existing with one. I have caused two lenses to be ground, one a double convex, the other a meniscus, as Dr. Wollaston directs, of the same diameter, nearly four inches, and focus twenty-two inches, which experimentally verify the correctness of my observations, and which any intelligent person may inspect, by application at our manufactory, 30, Holborn.

The following quotations may, to some of your readers, better corroborate the truth of my remarks:

"If the side were concave (of a plano) so that the lens became a meniscus, there is no proportion of the radii or position of the lens, with regard to the radiant, but what will give the aberration greater than the plano-convex in its best position; and since this was first observed by opticians, the meniscus began to lose ground in the construction of optical instruments, and is now quite rejected." Martin's Elements of Optics, 1759, page 29.

An oblique pencil of rays has its focus a little nearer the lens (double convex) than a direct pencil. Cor. fig. 2.

This proposition holds good in a concave lens, and also in a meniscus, as well as in a convex one. Emerson's Optics, page 124, prop. 24.

"When

"When parallel rays fall upon a plane side of a plano-convex glass, the aberration of the extreme ray, which is $\frac{9}{2}$ of the thickness, is less than the like aberration caused by any meniscus glass whose concave side is exposed to the incident ray.

"When the said glasses have their convexities turned to the incident rays, the aberration of the extreme ray in the plano-convex, which is now but $\frac{7}{6}$ of its thickness, is less than the like aberration of any meniscus in this position.

"The best of all double concave glasses has the semi-diameters of its first and second concavities as 1 to 6; and consequently this is the best figure of a glass to help short-sighted persons, as the double convex one of the like figure is the best for spectacles." Smith's Optics, art. 661, 662, 665.

"For since a meniscus, unless the surfaces of it are parallel to one another, has the same effect either that a convex lens or a concave one would have, all the cases of diverging or converging rays that are refracted by it will be the same with those already explained in the instances of convex or concave lenses." Rutherford's Philosophy, vol. i. page 286.

"A plano-convex glass, with its $\left\{\begin{matrix}\text{convex}\\ \text{plane}\end{matrix}\right\}$ side towards the incident parallel rays, has less aberration than any meniscus with its $\left\{\begin{matrix}\text{convex}\\ \text{concave}\end{matrix}\right\}$ side exposed to parallel rays. Whence it necessarily follows, that that meniscus is best, which approaches nearest in shape to a plano-convex lens." Harris's (of the Mint) Optics, 1776, page 67*.

The sort of French angle of reduction that Dr. Wollaston has given to obtain geometrically, but nearly, the radii of a meniscus for a given focus, will be useless to the workman, as he already knows by a very short arithmetical operation, how to obtain exactly such radii in half a minute's time, or a tenth part of the time necessary to construct that problem: by Gunter's sliding rule the time would be still shorter.

The combination of using two glasses in ordinary simple microscopes, or hand magnifiers, to diminish the errors arising from the spherical figure of one glass, was known to Sir Isaac Newton, and to successive opticians. That late

* So sensible have some optical glass-grinders been of the impracticability and insufficiency of the meniscus glasses of very short foci for spectacles, that I have in my possession some plano-convex and plano-concave glasses actually fitted in the frames, and sold for the *new periscopic glasses.*

excellent

excellent optician Mr. Ramsden, by the combination in the best position of two plano glasses, with their convex sides to each other, applied eye-pieces to his instruments with great advantage, to read off divisions of his circles, and magnify the wires of his telescopes with clear definition at the circumference of the field of view, the diameters of the glasses being no larger than the aperture of the tube. The same principle has since been advantageously applied to large object lenses for the lucernal microscope, by the late Mr. G. Adams and ourselves, where the diminution of light was of less consequence than indistinctness of the images. In many cases the combination of two convex lenses answers well; but the combining of two similar plano-convex lenses together, of superfluous diameter and thickness, and for the greatest defect or aberration, in the worst position to each other, and afterwards to palliate it with a small aperture, as shown in fig. 4, (Plate IV.) is such an anomaly or absurdity in optics as not to require any serious comment on my part. I shall only appeal to the least experienced constructor of microscopes, whether he does not know that the substitution of a double convex lens, of the diameter only of Dr. Wollaston's aperture, and of the same focus, would produce an image infinitely more perfect and vivid than the mutilated lens proposed by Dr. Wollaston.

From these remarks, I presume, there will be nothing to apprehend from the attempt of Dr. Wollaston to depreciate the excellence of the spectacles, camera obscuras, and microscopes, which have been constructed by the most eminent opticians of the day.

Yours, &c.

Holborn, April 13, 1813. WILLIAM JONES.

XXXIX. *M.* MONTGOLFIER's *Process for making White Lead. By Messrs.* CLEMENT *and* DESORMES *.

THE celebrated Montgolfier's process for the manufacture of white lead, being one of great simplicity, ought to be better known; and with this view we have drawn up the following description.

The first operation consists in forming the lead into sheets. He found from experience, that by running the melted metal on ticking, the sheets might be made of any thinness, and varied at will, by inclining the frame a little

* *Annales de Chimie,* No. 240.

more

more or less. The surface then becomes a little irregular, and full of points; which is favourable to the oxidation that follows. On this operation we need not insist, the process being already well known.

The second operation consists in oxidizing and carbonizing the lead. The following is the disposition of the apparatus:

M. Montgolfier had a common chemical reverberatory furnace, in which he burned charcoal. The chimney on its dome was four or five metres high, and, taking a horizontal direction, was introduced into an opening in the end of a cask (which lay on its side) a little above its centre. Some vinegar was put into the lower part of this cask, and towards the centre of its other end was adjusted another tube, equal to the chimney, and communicating by its other extremity with a large rectangular case in which were suspended the sheets of lead, alternately high and low, that the air might pass entirely over their whole surface. The other end of this case had an opening to allow the redundant gas to escape. The case had a cover, which could be removed at pleasure, for the purpose of placing the sheets of lead on small pieces of wood prepared to receive them.

The air from the furnace, being thus made to pass through the cask containing the vinegar, by communicating heat to the vinegar carries it off in vapour, and passes with it through the case containing the sheets of lead, which of course are exposed to the action of acetous acid, of carbonic acid from the combustion of the charcoal, and of oxygen and azote, or atmospheric air which has escaped the action of the fuel, and which may be augmented at pleasure by leaving holes towards the middle of the chimney to admit fresh atmospheric air. Thus are combined all the circumstances necessary to the production of carbonate of lead—oxygen, carbonic acid, vinegar, and heat.

In a short time the sheets of lead become charged with a coat of carbonate. If their entire conversion into carbonate at a single operation is not intended, they are withdrawn from the case, and suspended in water: the white lead readily detaches itself, and falls to the bottom. If the sheets are left till wholly converted into carbonate, still they must be put in water; and, besides, the deposit must be levigated to separate the metallic particles which may have escaped oxidation, and which would tarnish the white colour.

XL. *Essay on the medical Effects of Climates. From Dr.* Young's "Medical Literature."

[Concluded from p. 214.]

Mean of the greatest Variations of successive Days in each Month, for the Winter Months.

London, 1790–4, 6 mo.	11·5°
London, 1794 (greatest of all 15°)	10·7
Knightsbridge, Read, 1790-1 (greatest 23°)	16·3
Dawlish, 1794 (greatest $13\frac{1}{2}$°)	10·7
Lisbon, 1788 (greatest 11°)	8·7
Bermudas, 1790 (greatest 13°)	9·0
Montreal, 1778	4·0
Penzance, 1808–9. Nov. to March. (gr. 10°)	9·2
Sidmouth, 1800. Jan. to March. (gr. 16°)	10·9
Gravesend, 1787. Jan.	13·0
Ashover, Derbyshire, 1805. Jan.	13·5
Minehead, Atkins, 1782. Jan.	16.
Clifton, Feb. 1803, 9°, March, 13°, mean	11·

Mean Variation of successive Days for the Winter Months.

London, 1790–4, 6 mo.	3·62°
London, 1794	3·51
Knightsbridge, 1790–1	5·45
Dawlish, 1794	3·68
Lisbon, 1788	2·70
Bermudas, 1709, about	3·00
Montreal, 1778	13·2
Penzance, 1808–9. Nov. to March	2·80
Sidmouth, 1800. Jan. to March	3·32
Clifton, 1808. Feb. and March	3·55
Gravesend, 1787. Jan.	4·15
Ashover, 1805. Jan.	3·33
Minehead, 1782. Jan.	4·00

Mean diurnal Range for the Winter Months.

London, 1790–4, 6 mo.	13·0°
Sidmouth, 1800. Jan. to March	10·0
Clifton, 1808. Feb. and March (Lond. 16·2°)	11·4

Mean monthly Variation for the Winter Months.

London, 1793–6, 6 mo.	25·9°
Madeira, 1793–6, 6 mo.	12·6
Sidmouth, 1811. Jan. to March	34·
Clifton, 1803. Feb. and March (Lond. 36°)	31·

It does not appear that Devonshire possesses any decided advantages

advantages over London with respect to equability of climate, if we judge of the climate of London from the observations made at the apartments of the Royal Society only: but in so central a situation, the changes must be rendered much less sensible by the effect of the surrounding buildings; and they appear to be considerably greater at Gravesend, and greater still at Knightsbridge. In this respect, too, Penzance retains its superiority even over Devonshire. Lisbon seems to have a less variable temperature than any part of Great Britain; and in Madeira, to judge by the monthly variation only, the advantage in this respect appears to be still greater.

The greatest possible equability of temperature seems however to be obtained in a sea voyage to a warm climate, in which the variation seldom amounts to half as much as in the most favourable situation on shore, even on a small island; and in pulmonary cases, the motion of a ship would probably in general be rather beneficial than otherwise, while the fatigue of travelling in bad roads, and the danger of sleeping in damp beds, present an alternative by no means favourable to a journey by land.

The direction of the wind alone can seldom have any immediate effect on the salubrity of the climate, except by variously modifying its temperature, according to the seas or countries over which it blows. There is a method of computing the mean direction of the wind, which does not appear to have been hitherto adopted, but which affords a very simple and intelligible result, although somewhat laborious if extensively applied. It consists in finding the bearing and distance of a point, to which a light body would be carried by the wind in the course of the year, supposing the velocity to be constant, when its variations have not been ascertained by observation. It is obvious that the bearing of such a point will show at once the mean direction of the prevalent winds; and its distance, compared with the effect of a constant wind for the same time, as a unit, will indicate the degree in which those winds have prevailed.

Prevalence of Winds.

London, 1790–4	W.	9° S.	·234.
London, 1794	W.	33° S.	·188.
Dawlish, 1794	W.	6° S.	·466.
Lisbon, 1788	N.	1° W.	·315.

According to this comparison, it appears that the mean direction of the wind in Devonshire is somewhat more westerly than in London: and that the degree, in which such

such westerly winds predominate, is more than twice as great as in London: or, if we convert the measure into days, that the predominance amounted, in 1794, to 68 days for London, of a wind nearly W. S. W. and to 170 days for Dawlish, of a wind a little to the south of west.

The variations of the climate of the same place, with respect to mean temperature, are easily collected from the usual meteorological computations. Dr. Heberden has very successfully combated the common opinion respecting the superior salubrity of cold winters; it appears however that the winter which he particularly observed was more variable, as well as colder, than usual. Mr. Kirwan has attempted to account for the greater frequency of colds, which he supposes to occur in spring and in autumn, by the greater variability of the temperature at those seasons: but both the fact and the explanation are very questionable; for in reality the variations of temperature, if estimated by the total range of the thermometer within the 24 hours, are almost uniformly greatest in the hottest weather. In London, the greatest variations of successive days at the same hours in the morning are greatest in winter; in the afternoon, in summer; and although the latter are a little greater in April than in some of the succeeding months, the difference is by no means considerable.

Of the empirical evidence, which may be collected, respecting the medical effects of different climates, the most authentic is perhaps that which is derived from well regulated bills of mortality; since these documents ought to afford us a tolerable criterion of the general healthiness or unhealthiness of a place, from the proportion between the annual deaths and the population, and at the same time a pretty correct determination of the degrees in which different diseases are fatal. Thus, when we find that in Stockholm the annual deaths amount to $\frac{1}{19}$ of the population, in London to $\frac{1}{21}$, in the *Pays de Vaud* to $\frac{1}{45}$, and in some villages in different parts of Great Britain to $\frac{1}{60}$ only, we cannot hesitate to consider a residence in the country as generally more healthy, than in a metropolis similar to either of those cities; although it cannot fairly be concluded that the healthiness is precisely in the proportion which might be inferred from this comparison, until we have considered how far the effect of emigration to a great town may influence the apparent mortality. After the age of eight or ten, the probable duration of life may be estimated with sufficient accuracy, as Demoivre has very ingeniously shown, by assuming that, of a certain number of persons

born together, one will die annually until the whole number is become extinct; and it is well known, that this number may in common cases be supposed to be 86; so that at any given age, for instance 36, we may find the probable duration of life by deducting it from 86, and halving the remainder, which will give us 25 for the estimate required; and if this law were universally true from the time of birth, it is easy to show that the mortality in a metropolis would always be increased by the accession of settlers; so that if, for example, the whole population were supplied by settlers at 20, and all children were sent to a neighbouring village to be educated, the mortality of the town, instead of $\frac{1}{43}$, would become $1 : (43-10) = \frac{1}{33}$, and that of the village would be $1 : (86-10) = \frac{1}{76}$; and that any partial changes of a similar nature would cause a smaller alteration of the apparent salubrity, in proportion to their extent. But the mortality during infancy is actually much greater than is assumed in the simple hypothesis of Demoivre; and from this circumstance, as well as from the frequent return of aged persons into the country, Dr. Price has inferred that emigration in general has no tendency to increase the mortality of cities. In reality, the question depends altogether upon the mortality which may be supposed to take place within the first year, which is often estimated at one third of the births; but nothing like this can well be expected to occur at any tolerably healthy place in the country; and on the whole it does not appear that Dr. Price's observations can by any means be admitted as conclusive. With respect to the evidence afforded by the prevalence of diseases, it has been observed by Dr. Gregory, that removing from a colder to a warmer climate may be beneficial, even in those diseases to which the inhabitants of the warmer climate are subject; but if they appeared to be equally or more subject to any disease than the inhabitants of the colder, there would surely be little encouragement for the change: for instance, in a person supposed to be liable to diseases of the liver, it would surely be injudicious to undertake a voyage to a hot climate, with a view of avoiding the chance of taking cold, since the well known frequency of hepatitis, in such climates, would much more than counterbalance any prospect of advantage from the change.

The frequency of consumptions is decidedly greater in cold than in hot climates, but not by any means in exact proportion to the depression of the mean temperature. The principal situations, that require to be compared with the metropolis,

metropolis, as a standard, are the south of England, the south of Europe, the islands of the Mediterranean, Madeira, and the West Indies.

There do not appear to be any precise accounts of the proportionate mortality from consumption at any place upon the southern coasts of this island, on a scale sufficiently extensive for the comparison; but there is abundant reason to think that such a report would be greatly in favour of the salubrity of these coasts, more so indeed than any conclusions, that we should be at all authorised to form, from such thermometrical observations as have hitherto been compared. A greater number of registers is still wanting to obtain sufficient evidence for the inquiry: and it would be desirable that some journal should be kept at one of the Scilly islands, as a situation fully exposed to the influence of the sea air; for there can be little doubt that, for equability of temperature, a very small island must have great advantages above every other situation on shore. But in the present state of our knowledge on this subject, although we are fully justified in recommending a residence in Devonshire or Cornwall as advisable in a certain stage of consumption, it does not appear that any meteorological observations will authorise us to represent the advantages, to be gained by such a residence, as by any means equivalent to those which may be found in remoter situations; nor that the empirical testimony, derived from accounts of the comparative prevalence of the disease, is at all so clear, or so firmly established, as to make up for the want of evidence of a great and decided superiority of the climate.

In the south of Europe, the situations which have been most frequented are Lisbon, or some other part of the peninsula, the neighbourhood of Montpelier, and different parts of Italy. In Spain, and probably in Portugal, consumption is said to be not common, but by no means wholly unknown; and whether from accident, or from causes which are likely to have a constant operation, the climate of Portugal has certainly failed, in a number of instances, of producing any material benefit, where there has been apparently a very fair chance for the patient's recovery. With respect to the south of France, it is perhaps sufficient to remark, that the general proportion of deaths from consumption at Marseilles is fully as great, as the greatest which has been observed in London, where, according to Dr. Heberden's remark, its prevalence has of late years been so much increased. In Italy the disease appears to be decidedly less frequent: and there is no reason to doubt but that, in the

southern

southern parts of that country, there may be situations approaching in their climates to those of the neighbouring islands.

It is however highly probable that some of these islands possess very considerable advantages over almost every part of the continents which surround them, at least as far as we can judge by their affording a climate of that description which seems to be the most desirable; for actual experience will not allow us to be too confident of obtaining success, even from a residence in these. Dr. Domeier informs us, in his very interesting account of the island of Malta, that the thermometer seldom varies here more than 6° in the 24 hours, or stands below 51°, even in the depth of winter; while in Lisbon he has seen ice, and both ice and snow in Naples; besides that, in these two cities, the difference between day and night often amounts to 20°. If an invalid leaves England in the middle of August, the voyage lasts about a month, and is often of itself highly beneficial, so that he arrives at Malta, in time to be fully prepared to be further benefited by the mild winter: it appears, however, from the more particular account which Dr. Domeier elsewhere gives of the temperature, that it continues throughout October rather higher than is altogether desirable, being seldom below 70° throughout that month; and in a country where there is scarcely any visible foliage, walls occupying universally the place of hedges, this cannot be a matter of perfect indifference.

In Madeira, though the thermometer attached to a building is seldom found below 54°, there are frequently cold winds, snow, or more commonly something intermediate between snow and hail, often falling on the mountains, at the height of 1000 feet above the sea, and at still greater elevations sometimes lying undissolved till July: and this imperfect kind of hail falls occasionally even on the low grounds. The island is probably a more agreeable residence than Malta: but it seems very doubtful whether it possesses any determinate advantage over it with respect to climate; and it is not impossible, that some other islands in its neighbourhood may afford a greater equability of temperature. We have however a more established experience of its beneficial effects in pulmonary diseases than of almsot any other situation. Dr. Adams says that, "in cases of tubercular or scrofulous consumption, if the patient does not saunter away his time after you have advised him to leave England, we can with certainty promise a cure." (Med. Phys. Journ. Apr. 1800.) This true English consumption he

he thinks is not to be found in Madeira, while the catarrhal affection, which somewhat resembles it, though without purulent expectoration, is not uncommon, and may be fatal if neglected or improperly treated. Dr. Gourlay agrees with Dr. Adams, in his report of the general benefit derived from the climate of Madeira, by consumptive persons going to it from colder countries, to pass the winter in the island, and of the frequency of catarrhal affections among the inhabitants; but he strongly insists that genuine consumption is also very common and very fatal. There can however be little doubt, from the concurrent testimony of the majority of observers, that the climate of Madeira is extremely salubrious, and that consumptions, though they may sometimes occur, are comparatively rare.

In the West Indies, it is agreed by all authors, that consumptive affections are almost unknown, and that scrofula in all its forms is uncommon; while the inhabitants of the West Indies, coming into a colder climate, are peculiarly liable to the attacks of these diseases. Dr. Hunter, however, observes, that notwithstanding this exemption in favour of the natives of the West Indies, a residence in this climate appeared to him to be of no manner of advantage to persons who were already affected by incipient consumptions when they arrived there. We cannot doubt the accuracy of this evidence, as far as regards the facts which came immediately under Dr. Hunter's observation; they principally related to the military, who perhaps laboured under some peculiar disadvantages: but other practitioners have given much more favourable reports of the events of cases, in which they have made trial of the effect of a residence in this climate; and if we may be allowed to draw any inference from the qualities of a climate, as indicated either by the thermometer, or by its effects on the constitutions of the inhabitants, there can be little doubt that a residence in Bermudas, in a temperate and sheltered part of Jamaica, or in some other of the West India islands, together with the equable qualities of the sea air, to which the patient must be exposed during the voyage, must present every advantage, towards the recovery of a consumptive person, that climate alone can possibly bestow.

In other diseases, the effects of climate are perhaps less exclusively beneficial; although it appears that gouty persons often derive considerable benefit from a residence in the hottest countries, as in the East Indies, or at Ceylon in particular. Dr. Gregory seems to be persuaded that life may be lengthened, and the inconveniences of old age retarded

tarded or mitigated, by repeated emigrations into warmer and warmer climates, after the age of 50 or 60, according to circumstances: and he thinks that even posterity may be benefited by an emigration of this kind.

In whatever situation the residence of an invalid may be fixed, it is of no small importance that the aspect and exposure of the house, which he occupies, should be selected with a view to the qualities of climate which he is desirous of obtaining. We have an illustration of the truth of this remark, in an observation recorded by Dr. Carrick, respecting the influenza of 1803. "One of the most open and exposed of the buildings on Clifton hill is Richmond terrace, which forms three sides of a parallelogram, fronting respectively the east, south, and west; on the east side, not one family, and scarcely an individual, escaped the complaint; while on the south side, a great majority, both of persons and families, in all other respects similarly circumstanced, escaped it entirely." Such facts as these are among the few which afford solid grounds for medical reasoning; and they deserve the more attention, as they relate to circumstances of continual occurrence, and of perpetual influence on our health and comfort; and in proportion as both the medical and the meteorological sciences become founded on a firmer basis, it cannot be doubted that their beneficial effects will be more and more experienced, as well in the preservation of health, as in the treatment and cure of diseases.

TABLE OF THE ANNUAL MORTALITY

Of the different Counties of Great Britain, according to the Returns of 1811.

Middlesex	1 in 36	Lincoln	1 in 51
Kent	41	York, N. R.	51
Warwick	42	York, W. R.	51
Cambridge	44	Denbigh	52
Essex	44	Nottingham	52
Surry	45	Northampton	52
York, E. R.	47	Somerset	52
Huntingdon	48	Stafford	52
Lancaster	48	Worcester	52
Buckingham	49	Berks	53
Southampton	49	Flint	53
Mean of England	49	Glamorgan	53
Chester	50	Northumberland	53
Durham	50	Rutland	53
Norfolk	50	Suffolk	53

Brecon

Brecon	1 in 54	Devon	1 in 58
Cumberland	54	Hereford	58
Westmoreland	54	Mean of Wales	60
Wilts	54	Gloucester	61
Hertford	55	Carmarthen	62
Oxford	55	Cornwall	62
Sussex	55	Merioneth	62
Bedford	56	Montgomery	63
Derby	56	Monmouth	64
Radnor	56	Pembroke	64
Dorset	57	Carnarvon	67
Leicester	57	Anglesey	72
Salop	57	Cardigan	73

It is obvious, that those counties which contain large manufacturing towns exhibit a mortality wholly independent of their climate, as is exemplified in the case of Warwickshire; while the natural salubrity of others, for instance Cornwall, is probably rendered more conspicuous by their exemption from sedentary employments.

XLI. *On the Aurora Borealis.* *By Mr.* B. M. Forster.

To Mr. Tilloch.

Sir,—Being desirous of calling the attention of meteorologists and other persons to that beautiful meteor the aurora borealis, or northern lights, in order that they may observe it accurately, I beg the insertion of the following remarks in your Magazine as soon as may be convenient.

For several years past, these lights have made their appearance very seldom in this part of the island. Whether they have been seen as usual in the more northern counties I wish to be informed. They are, I understand, very brilliant at times in the Shetland islands, where they are called the Merry Dancers, from their rapidity of motion.

The celebrated Dr. Halley remarks of the aurora borealis as follows (see Phil. Trans. Mott's Abridgement, vol. ii.):

"This was the only sort of meteor I had not as yet seen, and of which I began to despair, since it is certain it hath not happened to any remarkable degree in this part of England since I was born; nor is the like recorded in the English annals since the year of our Lord 1574, that is above one hundred and forty years ago, in the reign of queen Elizabeth."

In the same paper, Halley afterwards mentions "one of

small duration seen in Ireland, by Mr. Neve, on the 16th of November 1707;" also "one of short duration seen near London, a little before midnight, between the ninth and tenth of August 1708."

It is, I believe, the opinion of some people, that this phænomenon did not appear in England until the 6th of March 1716, when it was very remarkable; a particular account of which is given in the paper of Dr. Halley above mentioned. Since that period it has been frequently observed, I understand, until of late.

It occurred to me some time ago, that having had a good deal of lightning lately, and very little of the aurora borealis, the disappearance of the one and appearance of the other might have some connection; and I find in the Gentleman's Magazine for January 1751 the same idea. Mention is there made of an aurora borealis which was seen in South Carolina in April 1750, and that there had been but very little thunder and lightning for several months; and an observation made, that had there been as much as usual, they would not have been witnesses of the aurora borealis in that part of the world.

This appearance is generally, and in my opinion with good reason, imagined to be occasioned by the electric fluid darting about in the atmosphere. Dr. Halley suspected it was caused by magnetical effluvia; for he remarks that this "subtile matter freely pervading the pores of the earth, and entering into it near its southern pole, may pass out again into the ether at the same distance from the northern;" and that "this matter may, by the concourse of several causes very rarely coincident, and to us as yet unknown, be capable of producing a small degree of light, perhaps from the greater density of the matter, or the greater velocity of its motion, after the same manner as the effluvia of electric bodies by a strong and quick friction emit light in the dark, to which sort of light this seems to have a great affinity."

Dalton in his Meteorological Essays, published some years ago, if I mistake not, remarks that the beams of the aurora lie in the same direction as the magnetic needle points, and that when they form a canopy of light, the vertex of it is at the magnetic pole of that place. I do not know whether this notion is original or not, but it is well worth the attention of philosophers. In the Gentleman's Magazine for January 1750, is an account of a remarkable aurora borealis seen in Northamptonshire, the coruscations of which "met not in the zenith, but in a point about

nineteen

nineteen degrees southward in the meridian." The *dip* of the magnetic needle being about 72 degrees, the southern end would consequently be inclined from the zenith 13 degrees.

This very near coincidence of situation as to altitude seems in some degree to strengthen Dalton's idea. What the horizontal position of the needle was in the year 1750, I do not know; but the expression "in the meridian" may not mean exactly so. I do not mean to offer an opinion on the connection between electricity and magnetism, but much wish for accurate statements of facts respecting the aurora. It is much to be wished that journals of the variation or declination of the magnetic needle were kept in various places, and transmitted to some periodical publication. The subject of magnetism is not in my opinion sufficiently attended to by philosophers. A good *variation needle*, as it is called, is not, I believe, a very expensive instrument, and might no doubt in many places be fixed tolerably secure from being shaken. Might not a *dipping needle* be constructed also, at a moderate expense, sufficiently exact for general use?

Would not the terms *horizontal magnetic needle* and *vertical magnetic needle* be preferable to *variation* and *dipping needle?* When the *situation* (or *direction*) of the magnetic needle is spoken of, the expression should include both the horizontal and vertical situation, that is, the azimuth; and *depression*, if we speak of the northern end, in these parts of the world.

I remain, &c.

March 18, 1813. B. M. FORSTER.

XLII. *On Bread made from a Mixture of Wheat Flour and Potatoes. By* H. B. WAY, *Esq. of Bridport Harbour**.

SIR,—I HAVE sent to the Society of Arts, &c. a loaf of bread made from a mixture of wheat flour and potatoes. The principle I have adopted from a publication of Edlin's, and I have now got it in such perfection, that I and my family prefer it to bread made wholly of wheat flour. It has the valuable property of keeping many days longer in

* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, for 1812.——The thanks of the Society were voted to Mr. W. for this communication, and for some excellent bread which he furnished to the Society, made from a mixture of Wheat Flour and Potatoes.

a moist

a moist state, which, in the country, where it is impossible to get fresh bread or yeast every dav, and where persons can perhaps only conveniently bake once a fortnight, is a very great advantage. I had many prejudices to encounter in the first attempts I made, and I think great merit is due to my servant Hannah Peters, for her perseverance and success, both in the making of it, and management of my oven in baking it, as both she and my neighbours were originally much prejudiced against my experiments in this line. I annex, for the Society's inspection, a statement of the cost and saving by the use of potatoes, and I hope, by degrees, this method will be extensively practised. I am sure, if the subject is noticed in the Society's volume, it will greatly contribute thereto. This is the second year that I have constantly used this mixed bread, from the latter end of October to the latter end of May; and I assure you that it is a matter of great regret to my whole family, when, from the scarcity of potatoes, we commence the use of bread made wholly from wheat.

I am very respectfully, dear sir,

Your obedient humble servant,

Bridport-Harbour, March 10, 1812. H. B. Way.

To C. Taylor, M.D. Sec.

Process for making Bread from Potatoes and Wheat Flour, as practised under the Direction of H. B. Way, *Esq. March* 10, 1812.

Sixteen pounds of potatoes were washed, and when pared weighed twelve pounds. After boiling they weighed thirteen pounds, and were then mixed, whilst warm, with twenty-six pounds of flour: the potatoes were bruised as fine as possible, and half a pound of yeast added. Four quarts of warm water were added to the mixture of potatoes, yeast and flour, and the whole well kneaded together, and left two hours to rise, and then weighed forty-six pounds and four ounces. The whole made six loaves and two cakes, which were baked at two separate times, in my iron oven, each baking taking two hours. The six loaves and two cakes, the day after being baked, weighed forty pounds and twelve ounces.

The oven is made of wrought iron on Count Rumford's plan, to heat from a separate fire-place. The time from the fire being lighted till the bread was baked at twice, was five hours, in which time six pounds of Walls-end coals and three pounds of cinders were consumed, besides a small quantity of wood used merely to light the fire.

Expenses

Expenses of Bread made from a Mixture of Potatoes and Wheat Flour, and Comparisons in Price with Wheaten Bread.

March 10, 1812.—16 lbs. of potatoes pared and boiled, weighed 13 lbs.; 4 lbs. allowance for interest and loss on the stock bought in October 1811, say 25 per cent., makes,

	s.	*d.*
20 lbs. of potatoes, at 6*s.* 6*d.* per sack of 240 lbs. the actual price when bought, October 1811,	0	6½
26 lbs. of fine flour, at 5*l.* per sack of 280 lbs. ..	9	3½
Half a pint of yeast	0	2
6 lbs. of coals, at 2*l.* 18*s.* 6*d.* per chaldron, of 2808 lbs.	0	1½
5 lbs. of cinders, and wood for lighting fire	0	1½
	10	3
40 lbs. 12 oz. of bread at the above date, at 1*s.* 4*d.* the quartern loaf, of 4 lbs. 5 oz. 8 drams, would have been	12	6
Leaves a saving of	2	3

	lbs.	oz.	drams.
26 lbs. of flour at the rate of 80 loaves, of 4 lbs. 5 oz. 8 drams each, to the sack of 280 lbs. would only have made	32	4	4
Gain in bread by 16 lbs. of potatoes, is more than half a pound of bread for each pound of potatoes	8	7	12
	40	12	0

The iron oven has been in use more than 15 years, it is 20 inches deep, 16 inches wide, and 16 inches high; and has been recently fresh set to heat from a separate fire-place, which is 10½ inches deep, 7½ inches wide, and 7 inches high, the bars of the fire-place 14 inches from the bottom of the oven.

Mr. Way's bread had been sent from Bridport Harbour to the Society on the 10th of March 1812; and had been examined and tasted at sundry times by members of the Society, from the 12th to the 26th of March, so that the greatest part of the loaf had been eaten. What remained, on the 26th, had every appearance of bread made wholly from wheaten flour well fermented, and well tasted, without being in the least mouldy or stale, though it had been baked fourteen days. It appeared to the Committee to be a very successful mode of making bread, and that it might tend

tend to lessen the consumption of flour, an object of considerable national importance.

_ Persuaded that in domestic œconomy Mr. Way's communication may prove useful to many families, we have given it a place in our pages. We have only to add, that so far as this *mixed* bread may be considered as preferable to that from flour, we have reason to believe that many of the good inhabitants of London, and its vicinity, have for a considerable time been enjoying the benefit—to the great emolument of many honest bakers.

If the absurd practice of regulating the price of bread must be continued, would it not be equitable that the magistrates, in fixing the assize, should in future be directed, by law, to take into their consideration not merely the market price of flour, but also of potatoes?—EDIT.

XLIII. *On Solids of greatest Attraction, or Repulsion.*

To Mr. Tilloch.

SIR,—SUCH cylindrical portions of a sphere as I treated of in my former papers, lead to interesting results, respecting solids of greatest attraction; as will be seen in the third of the following propositions. I might have added that proposition to my last letter, but thought it would be better to connect it with *a general theory of solids of greatest attraction.* I am, sir,

Your obedient servant,

X. Y.

PROP. I.

Conceive a particle of matter, m, to be placed at the origin of the three rectangular coordinates x, y, and z:—What must be the nature of the solid (of given mass) which shall exercise the greatest possible attraction on the point m, in the direction of x: the force being as $\phi(x, y, z)$* a function of x, y, and z; and the density as some other function $\Delta(x, y, z)$ of the same quantities.

It is easy to see, that the force which the solid exercises, on the point m, in the direction of x, is $\iiint \frac{x.\phi(x,y,z).\Delta(x,y,z)\,\dot{x}\,\dot{y}\,\dot{z}}{(x^2+y^2+z^2)^{\frac{1}{2}}}$; its mass is $\iiint \Delta(x,y,z)\dot{x}\,\dot{y}\,\dot{z}$.

* This expression, for the force, need not be restricted to such functions as are positive for all values of x, y and z; we may extend the inquiry to those cases in which the particles exercise a repelling force; or an attracting or repelling force according to their situation.

Therefore,

Therefore, C representing a constant quantity, the expression $\iiint \frac{x.\phi(x,y,z).\Delta(x,y,z)\dot{x}\dot{y}\dot{z}}{(x^2+y^2+z^2)^{\frac{1}{2}}} + C\iiint \Delta(x,y,z)\dot{x}\dot{y}\dot{z}$ is to be a maximum; and, by the method of variations, we find, for the equation of the superficies, $\frac{x.\phi(x,y,z).\Delta(x,y,z)}{(x^2+y^2+z^2)^{\frac{1}{2}}} + C.\Delta(x,y,x) = 0$, or $\frac{x\phi(x,y,z)}{(x^2+y^2+z^2)^{\frac{1}{2}}} + C = 0 \ldots\ldots\ldots(1.)$

Cor. 1. We learn, from the preceding analysis, that the figure of the solid is independent of the law of density, *in all cases*. This leads me to observe, that although Mr. Playfair, in the Edinburgh Transactions, restricts his problem to the case of homogeneity, there appears to be nothing in his ingenious manner of treating the subject, which renders such a supposition necessary. For, when we are directed to take a small portion of matter from a point at C, and place it at another point D, it may be conceived to be contracted or dilated, at this latter point, as any variable law of density may require, without making any difference in the reasoning, or in the result.

Cor. 2. If we suppose $\phi(x,y,z)$ to be a function of the distance, or of the form $\phi(x^2+y^2+z^2)$, equation (1) takes the form $x=\psi(x^2+y^2+z^2)$, which is the general equation of solids of revolution: see *Monge* "Application," &c. p. 18.

SCHOLIUM.

Although it appears from what has been shown, that when the force is a function of the distance, the solid of greatest attraction must be a solid of revolution; yet the converse is by no means true;—that the particles must necessarily act with a force, which is some function of the distance, in order that the solid of greatest attraction may be a solid of revolution. Thus, if we want the figure of the solid when the force, or $\phi(x,y,x) = \frac{\sqrt{x^2+y^2+z^2}}{d-\frac{y^2+z^2}{x}}$, equation (1) becomes $\frac{x}{d-\frac{y^2+z^2}{x}} + C = 0$, or $x^2 + C(dx - y^2 - z^2) = 0$, evidently the equation of a solid of revolution round the axis of x. Let a be the value of x when y and $z = 0$, then we have $C = \frac{-a}{d}$, and by substituting this

value

value the equation becomes $ax-x^2=\frac{a}{d}(y^2+z^2)$, which belongs to an ellipsoid of revolution, or a sphere if $d=a$. Again, if $\phi(x,y,z)=\frac{\sqrt{x^2+y^2+z^2}}{x+B(x^2+y^2+z^2)}$, we find the solid to be a sphere.

PROP. 2.

It is required to solve the reverse problem, or to find the force when the figure of the solid of greatest attraction is given.

By equation (1) $x=\frac{C\sqrt{x^2+y^2+z^2}}{\phi(x,y,z)}$: Suppose then, that, from the nature of the given solid, $x=F(x,y,z)$, we have by making these values equal $F(x,y,z)=\frac{C\sqrt{x^2+y^2+z^2}}{\phi(x,y,z)}$; whence, $\phi(x,y,z)=\frac{C\sqrt{x^2+y^2+z^2}}{F(x,y,z)}$.

Cor. It is plain that we may give $F(x,y,z)$ a variety of forms for the same solid; and, consequently, that there may be various laws of force, $\phi(x,y,z)$, which give the same solid for the solid of greatest attraction.

If the solid be of revolution, there will be *one* of these laws of force which is a function of the distance*; and to the finding this law I shall confine myself in the following examples.

Ex. 1. What function of the distance must the law of force be, when the solid of greatest attraction is an ellipsoid of revolution, whose axis of revolution coincides with that of x, and terminates at the attracted point m?

Let a be this axis, b the other, the equation of the superficies is $\frac{b^2}{a^2}(ax-x^2)=y^2+z^2$; or, putting $D^2=x^2+y^2+z^2$, $\frac{b^2}{a^2}(ax-x^2)+x^2=D^2$, whence $x=a\sqrt{\frac{b^4}{4(a^2-b^2)^2}+\frac{D^2}{a^2-b^2}}-\frac{ab^2}{2(a^2-b^2)}=F(x,y,z)$ by substituting which we find the force; or, $\phi(x,y,z)$

$$\propto\frac{D}{a\sqrt{\frac{b^4}{4(a^2-b^2)^2}+\frac{D^2}{a^2-b^2}}-\frac{ab^2}{2(a^2-b^2)}}\propto\frac{D}{\sqrt{1+\frac{4(a^2-b^2)}{b^4}D^2}-1}.$$

If the spheroid be oblong, this law of force is is always possible.

* Not, however, always possible, as we shall see.

Ex.

Ex. 2. What function of the distance must the law of force be, when the solid is half of an ellipsoid of revolution, with its centre at the point m, and its axis of revolution coinciding with that of x?

The equation of the solid now (if a and b denote the halves of what they did before) is $\frac{b^2}{a^2}(a^2-x^2)=y^2+z^2$; or, $\frac{b^2}{a^2}(a^2-x^2)+x^2=\text{D}^2$; whence, $x=\frac{a\sqrt{\text{D}^2-b^2}}{\sqrt{a^2-b^2}}=\text{F}(x,y,z)$, and the required force, or $\phi(x,y,z)\propto\frac{\text{D}}{\sqrt{\text{D}^2-b^2}}$.

If the spheroid be oblate, D is always less than b, and the law of force here found will be impossible: or, in other words, no force, which is a function of the distance, will have the *oblate* spheroid for a solid of greatest attraction, with respect to a point at its centre.

But a portion of an *oblong* spheroid thus situated, may be a solid of greatest attraction, with this law of force, provided there be a distance greater than b between the solid and attracted point.

Prop. 3.

The force being inversely as the square of the distance, what must be the base of a homogeneous cylinder erected perpendicularly on the plane of x and y, and intercepted, above this plane, by a given curve surface, in order that the intercepted cylindric portion may exercise the greatest possible attraction, on a point m at the origin of the coordinates, in the direction of x; its mass being given?

The attraction is $\iint\frac{xz\dot{y}\dot{x}}{(x^2+y^2)(x^2+y^2+z^2)^{\frac{1}{2}}}$; the mass is $\iint z\dot{y}\dot{x}$; therefore, if $z=\text{f}(x,y)$ express the nature of the given surface, the following expression must be a maximum; viz.

$\iint\frac{x\,\text{f}(x,y)\,\dot{y}\,\dot{x}}{(x^2+y^2)(x^2+y^2+\text{f}(x,y^2)^{\frac{1}{2}}}+\text{C}\iint\text{f}(x,y)\;\dot{y}\,\dot{x}$; so that the equation of the required curve, bounding the base, is

$$\frac{x}{(x^2+y^2)\left(x^2+y^2+\text{f}(x,y)^2\right)^{\frac{1}{2}}}+\text{C}=0.$$

Ex. 1. Let $\text{f}(x,y)$ be $\text{f}(x)$, a function of x only, and we get the same result as in Prop. 34 of a late paper in the Phil. Trans. which is only a particular case of this.

Ex. 2. Suppose the intercepting surface to be a sphere, to radius

radius r, and the attracted point and origin of the coordinates to be at its centre. Then $z^2 = \mathrm{f}(x, y)^2 = r^2 - x^2 - y^2$, and the equation of the curve becomes $x = \acute{C}\ (x^2 + y^2)$: which belongs to a circle having the attracted point at the extremity of its diameter. So that the portion, cut out by the cylinder, in Viviani's celebrated problem, is a solid of greatest attraction of this kind.

Ex. 3. The surface still being a sphere, if the attracted point, instead of being at the centre, is at the extremity of a diameter, which is also the axis of x, we shall have $z^2 = \mathrm{f}(x, y)^2 = 2rx - x^2 - y^2$; whence the equation of the curve is $\frac{x}{(x^2+y^2)\sqrt{2rx}} + C = 0$, or $x = \acute{C}\,(x^2 + y^2)^2$; which is the same curve which generates, by its revolution, the solid of greatest attraction, when the force is inversely as the cube of the distance. Vide Ed. Trans. vol. vi. p. 203.

In the first proposition, the *mass* of the solid was supposed to be given, in which case it appeared that the figure of that solid is independent of the density: but if it is the *volume*, instead of the mass, that is given, the case will be different.

PROP. 4.

It is required to solve Prop. 1. but with this difference, that now the volume $\left(\iiint \dot{x}\,\dot{y}\,\dot{z}\right)$, and not the mass, is supposed given.

We find immediately, for the equation of the solid of greatest attraction,

$$\frac{x\,\phi(x, y, z)\,\Delta(x, y, z)}{(x^2 + y^2 + z^2)^{\frac{3}{2}}} + C = 0.$$

If the density is constant, this, of course, enters into the first proposition; but, in other cases, there may be an infinite variety of functions of the distance which representing the law of force, will give the same solid of greatest attraction; provided a suitable density be supposed.

Ex. 1. What laws of force and density will give the solid of greatest attraction, of this kind, a sphere? It is plain we have only to satisfy the equation $\phi(x, y, z) \times \Delta(x, y, z) = \frac{1}{(x^2+y^2+z^2)^{\frac{1}{2}}}$. Thus, if the force is to be a function of the distance, we may make $\phi(x, y, z) = \frac{1}{\mathrm{f}(x^2+y^2+z^2)(x^2+y^2+z^2)^{\frac{1}{2}}}$, $\Delta(x, y, z) = \mathrm{f}(x^2 + y^2 + z^2)$, or the reverse.

Ex.

Ex. 2. If the equation of the solid be $Cx^2 = (x^2 + y^2 + z^2)^3$, the density may be $= \mathrm{f}(x^2 + y^2 + z^2)$, and the force $= \frac{1}{\mathrm{f}(x^2 + y^2 + z^2)(x^2 + y^2 + z^2)}$.

XLIV. *On a Composition forming a Substitute for Portland Stone. By Mr.* CHARLES WILSON, *of the Borough of Southwark*.*

SIR,—I BEG leave to lay before the Society instituted for the Encouragement of Arts, &c. a substitute for Portland-stone chimney-pieces, made by me, and no other person, at present, in this kingdom, and with such certificates of their utility as I trust will prove satisfactory.

I am most respectfully, sir,

Your very obedient servant,

CHARLES WILSON.

No. 35, Worcester-Street, Queen-Street, Borough of Southwark, Jan. 28, 1812.

To C. Taylor, M.D. Sec.

Mr. WILSON's *Process for Artificial Stone Chimney Pieces.*

TAKE two bushels of sharp drift sand, and one bushel of sifted slacked quicklime, mix them up together with as little water as possible, and beat them well up together for half an hour, every morning for three or four successive days, but never wet them again after their first mixture.

To two gallons of water, contained in a proper vessel, add one pint of single size, made warm; a quarter of a pound of alum, in powder, is then to be dissolved in warm water, and mixed with the above liquor.

Take about a shovel full of the first composition, make a hole in the middle of it, and put therein three quarters of a pint of the mixture of alum and size, to which add three or four pounds of coarse plaster of Paris; the whole is to be well beaten aud mixed together rather stiff; put this mixture into the wooden moulds of your intended chimney-piece, the sides, ends and tops of which moulds are made of moveable pieces, previously oiled with the following mixture.

Take one pint of the droppings of sweet oil, which costs

* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce,* for 1812.——The Society voted twenty-five guineas to Mr. Charles Wilson for this communication, and a model of such a chimney-piece is preserved in the Society's Repository.

 about

about one shilling the pint, and add thereto one pint of clear lime water, made from pouring boiling water on lumps of chalk lime in a close vessel till fully saturated: when the lime water becomes clear, it is proper to be added to the oil as above mentioned, and on their being stirred together they will form a thick oily mixture, or emulsion, proper to apply upon the moulds.

In forming the side or jamb of a chimney-piece, the mould is to be first half filled with the sand-lime and plaster composition, then two wires wrapped round with a thin layer of hemp, and which wires are nearly the length of the piece to be moulded, are to be placed in parallel lines, lengthways, in the mixture or composition in the mould, and afterwards the mould is filled up with more of the composition, and if there is any superfluous quantity, it is to be struck off with a piece of flat board.

The lid or top part of the mould is to be then placed upon it, and the whole subjected to a strong pressure from weighted levers or a screw press. The composition is to remain under this pressure for twenty or thirty minutes; the precise time necessary may be known, from examining a small specimen of the composition reserved purposely to determine the time it requires to harden and set firm.

The sides of the mould are to be held together by iron clamps and wedges.

The wires above mentioned answer a double purpose, by giving strength to the jambs, and retaining the whole mass together in case it should at any time be cracked by accident.

The chimney-pieces may be made either plain or fluted, according to the mould, and when moulded, they are finished off by rubbing them over with alum water, and smoothing them with a trowel and a little wet plaster of Paris.

A common plain chimney piece of this composition is sold at only seven shillings, and a reeded one at twenty-eight shillings, completely fitted up.

Certificates were received from the following Persons.

Mr. George Smart, of Ordnance Wharf, Westminster Bridge, who had tried these chimney-pieces for three years, and found them a valuable article. Mr. J. Willoughby, who had fitted-up nine rooms with these chimney-pieces, in York-street, Broadway, Westminster. Mr. William Simpson, Hackney-road, who had funished four rooms with

with them. Mr. Butler, Weymouth-place, Hackney, who had fixed them in sixteen rooms. Mr. Cherry, who had fixed them in eight rooms, near Cuckfield, in Sussex. The general tenor of the above certificates shows that they have found these chimney-pieces to answer the same purpose as those made of Portland-stone, and provided at half the expense.

XLV. *An Attempt to determine the definite and simple Proportions, in which the constituent Parts of unorganic Substances are united with each other.* By JACOB BERZELIUS, *Professor of Medicine and Pharmacy, and M.R.A. Stockholm.*

[Continued from page 205.]

IX. MURIATE OF LEAD.

1.) FIVE grammes of yellow oxide of lead were dissolved in muriatic acid in a glass flask; the product dried and melted in the flask, was 6 187 gr. of muriate of lead.

2.) Ten gr. of the yellow oxide afforded in a similar experiment 12·30: during the fusion, a little horn lead flew off with a visible vapour, the smell of which was not acid, but like that which is afforded by liquid metallic salts. Hence it follows that the muriate of lead consists of 81 or 80 82 of oxide and 19 or 19·18 of acid.

3.) Five gr. of muriate of lead, fused in a red heat, were dissolved in water impregnated with a little nitric acid, and precipitated by nitrate of silver. The precipitate, when fused, weighed 5·11 gr.; and this gives 19·13 of acid in 100 of the muriate of lead.

4.) The experiment was repeated, and afforded 5·09 of muriate of silver; whence we have 19·04 of acid.

According to these experiments, the muriate of lead consists of

	Muriatic acid	19·18	100·0
	Oxide of lead	80·82	421·4

If we calculate from the component parts of the sulphate of baryta, the sulphate of the protoxide of lead, and the muriate of baryta, we have 194 : 280 = 288 : 416 2. The calculation differs by 5·2 from the experiment; and although I have frequently repeated the processes, I have not been able to detect the source of the error. If, according to one of the experiments, we take 193 for the baryta by which 100 parts of sulphuric acid are saturated, we still have only 419 of oxide of lead to 100 of muriatic acid. The oxide, which saturates 100 parts of muriatic acid, con-

 tains,

tains, according to the experiment, 30·49, and according to the calculation, 30·1 of oxygen. Since 100 parts of muriatic acid take up the same quantity of oxygen in the oxide of lead as in the two degrees of oxidation of copper, the proposition already laid down is further confirmed by this agreement.

I must however here confess the existence of an irregularity, which I cannot yet explain, but which supposes an inaccuracy in some of those experiments which I thought the most unexceptionable.

A hundred parts of muriatic acid are saturated by 434·8 of oxide of silver, which contain 31·9 of oxygen. Now, since the analyses of the oxide and the muriate of lead, and especially that of the muriate of silver, seem to be of such a nature as to be susceptible of accuracy, and since they are also confirmed by other tests which serve as checks to verify them, I am utterly unable to discover on what the principal error can possibly depend. Does the salt of silver contain water? I have melted it in a red heat, without any loss of weight. Is the oxygen of the lead assumed below the truth? I refer to the experiments on the sulphate of lead, the second of which was occasioned precisely by this question; here it appeared that 100 parts of lead, with sulphuric acid, afforded exactly as much sulphate as 107·8 of the oxide of lead. Or, is the analysis of the muriate of lead inaccurate? This supposition is contradicted both by the result of the calculation, which agrees sufficiently well with the analysis, and by that of the precipitation with nitrate of silver. The difference of 1·41 of oxygen between the oxides of lead and of silver is indeed not very considerable; but it must depend on some unknown circumstance.

X. Iron and Sulphur.

It has long ago been demonstrated by Proust, that several metals may be combined with sulphur in two proportions, a maximum and a minimum. It appeared to me to be interesting to examine, how far inflammable bodies observe the same laws in their combinations with each other, as with oxygen. For this purpose I chose the sulphuret of iron, as being most easily subjected to analysis.

A. *Sulphuret of Iron at a Minimum.*

I mixed one part of pure iron, very nearly free from carbon, which had been rolled out to the thickness of a leaf, with three of pure sulphur, and heated them in a small glass retort with a receiver luted to it. When the sulphur had

had been distilled over, I ignited the mass, and as soon as the gas in the bulb of the retort had lost its yellow colour, I suffered the apparatus to cool. The mass had retained the form of the plate of iron; and when it was touched, some pretty thick shining scales fell off from the iron on which the sulphur had not acted. These scales had a crystalline fracture and a metallic appearance. In their entire state they were not attracted by the magnet, but they became magnetic when pulverised.

Two grammes of these scales, in large and regular pieces, were digested with aqua regia, till nothing remained undissolved, and the solution was precipitated by muriate of baryta. The precipitate afforded 5·38 gr. of ignited sulphate of baryta.

According to the experiments above related, 100 parts of sulphate of baryta contain 34 of sulphuric acid, and in this 13·795 of sulphur; so that 5·38 gr. give ·742 of sulphur, that is, 37·1 per cent. of the weight of the sulphuret of iron. (Mr. Hatchett, who has examined the magnetical pyrites, makes it 36·9: at the same time his mode of analysis, and the data assumed for his computations are such, that I can only consider our agreement, in this and the following analysis, as perfectly accidental.)

The liquid to which the muriate of baryta had been added was freed from the baryta by sulphuric acid, and then decomposed by caustic ammonia. The oxide, after ignition, weighed 1·82 gr. which gives 1·26 of iron. Here then is an excess of ·002 gr. [one thirtieth of a grain,] which may have depended on the supposition of too great a proportion of sulphur in the sulphuric acid, or perhaps on a slight inaccuracy of the weights employed. If we compute the quantity of sulphur from that of the iron, it will appear that 100 parts of iron took up 58·73 of sulphur; so that the sulphuret of iron at a minimum consists of

Sulphur	37	58·73
Iron . . .	63	100·00

If on the contrary we deduce the proportions from the quantity of sulphur determined, we shall have 58·88 of sulphur for 100 of iron, and 100 of the sulphuret will contain 37·1 of sulphur and 62·9 of iron.

B. *Sulphuret of Iron at a Maximum.*

In order to determine the proportions of this compound, I reduced again some of the scales to a fine powder, mixed them with finely pounded sulphur, and distilled the mixture in a small glass retort with a very gentle heat, as long as

 any

any sulphur passed over. The mass, when taken out, was still in the form of a powder, only that its colour was somewhat brighter, and it was still partly attracted by the magnet. It was not however soluble in muriatic acid. Two grammes of it were burnt in an open platina crucible, and left 1·4 gr. of red oxide of iron not at all magnetic, answering to ·97 of metallic iron. Consequently the remaining 1·03 gr. was sulphur; and 100 parts of iron had taken up 106·5 of sulphur. Since however the pyrites, which was formed, was still partly magnetical, I conjectured that it somewhat resembled the red oxide, which by too strong ignition has been reduced in a slight degree to the state of a protoxide, and for this reason is again subjected to the influence of the magnet.

I therefore distilled 20 gr. of very pure native pyrites in a small glass retort with a receiver. At first a trace of moisture passed over, which, when the experiment was concluded, had assumed the form of oily drops adhering to the receiver, and which I took for concentrated sulphuric acid; but by dilution with water this fluid became milk white, and not at all acid. It was therefore neither water nor sulphuric acid. I could not afford this substance any further attention; perhaps it was the alcohol of sulphur. The mass left in the retort was exposed for some time to ignition; it had lost 4·4 gr. of sulphur, which was collected in the neck of the retort and in the receiver. Of the remaining 15·6 gr., five were dissolved in nitric acid: these being evaporated to dryness, and ignited, in a platina crucible, left 4·3 of red oxide, which was not at all magnetical. Dissolved in muriatic acid, it left ·02 of silica. Hence we have 13·416 of red oxide for the whole mass, or deducting ·0625 for silica, 13·35, answering to 9·258 of metallic iron. There remain therefore 10·7 gr. for the sulphur. Consequently 100 parts of iron had been combined with 115·5 of sulphur; that is, with nearly twice the quantity which had been found in the sulphuret at a minimum.

I repeated the experiment with some select pieces of another specimen of pyrites. It was very finely powdered, roasted in the muffle of an assaying furnace, in a dish of platina, and in the mean time occasionally stirred with a hook of the same metal. Ten grammes of pyrites afforded me 6·67 of red oxide, not in the least magnetical, leaving ·07 of silica when dissolved in the muriatic acid. These 6·6 gr. of red oxide indicate 4·5775 gr. of pure iron, which, added to the quantity of silica, and subtracted from the whole weight, leaves 5·3525 for the sulphur. Consequently

100

100 parts of iron were combined with 117 of sulphur, and sulphuret of iron at a maximum [or super-sulphuret of iron] consists of

Iron	46·08	100
Sulphur	53·92	117

The combination of 100 parts of iron with $58\frac{3}{4}$ of sulphur in the former and in the latter case 117·2, that is ·3 parts less than a double portion, indicates some slight inaccuracy in one of the experiments. If we assume that the silica, found in the pyrites, existed in a metallic state, which is a very probable supposition, we must calculate on ·04 only of the base, or of silicium; and the sulphur combined with 100 parts of iron will be 117·5, that is precisely according to the calculation; neglecting, at least, the small quantity of sulphur which may have been combined with the silicium, if these bodies have any affinity for each other.

It may be considered as tolerably well established, that no other combinations take place between iron and sulphur than the two which are here examined. Yet we often find, in preparing the artificial pyrites, products which are differently constituted. This was the case, for example, in my analysis of sulphuretted hydrogen (Afh. ii. 86.) The sulphuretted iron, which I then employed for obtaining the gas, contained $36\frac{1}{4}$ of sulphur to 100 of iron. In general, I have found in the preparation of the sulphuret at a maximum by ignition in close vessels, that when the mass was not brought into fusion, the iron always retained a greater quantity of sulphur, than the sulphuret at a minimum contains. In two different experiments, I found this excess of sulphur pretty constant; in one, 100 parts of iron had retained 68·6, in another, 68·2 of sulphur. If in the preparation of the sulphuret we employ iron in excess, a portion of the metal is dissolved by the sulphuret, and the solution may vary by imperceptible gradations, like that of a salt in water. If this were not the case, the whole doctrine, which is supported by so many experiments, must be merely a groundless imagination. [The solubility of a metal and its oxides in the sulphuret, in all proportions, was observed by Proust in his experiments on metallic sulphurets, especially with respect to antimony.—*Gilbert.*]

We have seen that the sulphuret of lead, and in all probability that of copper also, become neutral salts by oxygenization. It is now to be inquired if the same is true of the sulphuret of iron.

 XI. SUL-

XI. Sulphate of Iron.

Some crystallized sulphate of iron, which had been obtained by dissolving sulphuret of iron in dilute sulphuric acid, was reduced to a coarse powder, first washed with water, then digested with a little spirit of wine, in order to separate the superfluous sulphuric acid, and dried with blotting paper. That which had crumbled having been rubbed and blown off, 10 gr. of this salt were exposed in a glass retort to a high temperature, short of ignition. They lost 4·63 gr. of water. I was in hopes that, if all the water were thus driven away, I should be able to compute, from the analysis of the dry salt, the quantity of oxygen in the protoxide which had entered into combination with the acid; but having repeated the experiment several times with different results, I found that a part of the acid escaped with the last portions of the water, being reduced to the state of sulphurous acid.

1.) Nine grammes of the crystallized and washed salt were dissolved in water, and mixed with the nitric acid in great excess, being boiled with it until the protoxide was fully oxidated: muriate of baryta was then added: the precipitate when washed and ignited weighed 7·685 gr., containing 2·613 of sulphuric acid, or 1·06 gr. of sulphur. Sulphuric acid was added, to throw down the superfluous baryta, and then caustic ammonia, which afforded a precipitate of 2·59 gr. of red oxide of iron, containing 1·796 of the metal. Consequently 100 parts of iron had been combined with 59 of sulphur.

2.) Ten gr. of sulphate of iron, treated in the same way, afforded 8·5 of ignited sulphate of baryta, and 2·87 gr. of oxide of iron. The former corresponds to 2·89 grains of sulphuric acid, or 1·172 of sulphur; the latter to 1·99 of iron. Consequently 100 parts of iron had taken up 58·9 of sulphur.

3.) In both these experiments, notwithstanding the excess of acid, the sulphate of baryta had attracted a portion of the oxide of iron, which gave it a yellowish tinge after ignition; I therefore varied the process, so as to separate first the iron and then the acid. Ten grammes of sulphate of iron afforded in this manner 2·935 of red oxide of iron, and 8·7 of sulphate of baryta: hence we have 2·055 gr. of iron, 2·958 of sulphuric acid, and 1·997 of sulphur; and 100 parts of iron again appear to be united to 58·9 of sulphur.

These experiments therefore completely demonstrate, that

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in the sulphate of the protoxide of iron, the sulphur and iron are in the same proportion as the sulphuret at a minimum.

The slight excess of ·15 of sulphur undoubtedly depends on some trifling error in the experiments, or in the data; perhaps, for instance, from having assumed a little too much sulphur in the sulphate of baryta.

Thenard has described six different combinations of iron with the sulphuric acid, (*Ann. Ch.* lvi. 59.) and among them a supersulphate of the protoxide. It is obtained by adding concentrated sulphuric acid to a solution of neutral sulphate of the protoxide. Although I had often seen salts precipitated from their solutions by acids, without receiving an excess of acid, for instance the muriates of baryta and of copper by the muriatic acid, yet I determined to examine the fact more accurately. The fine grained white salt, which I obtained in this manner, was freed from the acid which adhered to it by water and spirit of wine, then dried, dissolved, and decomposed. It appeared to contain exactly the same proportions of its component parts, as the neutral salt; and when it was evaporated in a retort, it afforded crystals of the same kind. Consequently Thenard's supersulphate is nothing more than the neutral salt, its whiteness depending only on the state of powder in which it is thrown down by the acid. Here therefore we could not expect to find a combination answering to the sulphuret at a maximum.

Thenard, who considers the neutral salt which I have examined as acidulated, although in a lower degree than that which is thrown down by the acid, describes a neutral sulphate of an emerald green colour, which is obtained when dilute acid is boiled with an excess of iron filings. The sulphate, which I employed in my analysis, had been boiled with iron filings as long as there was any action between them, and yet it was by no means this emerald green salt.

I now kept a solution of neutral sulphate of the protoxide of iron in a gentle heat for several days in an open vessel, and as it evaporated, I added water gradually. During this operation a yellow powder was precipitated. The salt which was left at last crystallized in oblique rhombs, and had an emerald colour. A part of this salt I took out; the remainder I boiled with nitric acid, and afterwards exposed it in a platina crucible to a heat approaching to ignition. This mass, freed from nitric acid, was dissolved in water; it deposited a red powder, which I afterwards found to be sub-

subsulphate of the oxide. The solution was evaporated to dryness, and the remainder was heated in a platina crucible, in order to expel all the water. It weighed 5·7 gr. When dissolved in water, it deposited some subsalt, of which the acid had been driven away with the water of crystallization, and which, after ignition, weighed ·24 gr. The solution in water was decomposed by adding to it first caustic ammonia, and then muriate of baryta. The red oxide after ignition weighed 2·16, answering to 1·498 gr. of pure iron: the salt of baryta 9·7 gr. which contained 3·3 of sulphuric acid, or 1·335 of sulphur. Consequently 100 parts of iron had been united with 89 of sulphur, that is, with half as much more, within ·5, as is contained in the sulphuret at a minimum. This gives for 100 parts of sulphuric acid 65·46 of oxide of iron, containing 20·1 of oxygen, or again, within ·19, the same quantity as we have already found to be appropriated to the base uniting with 100 parts of sulphuric acid. According to this analysis, the *neutral sulphate of the oxide of iron* consists of

Sulphuric acid	60·44	100·0
Oxide of iron	59·56	65·5

The yellow powder, which was deposited by the neutral sulphate of the protoxide during the digestion in an open vessel, which is considered as a neutralised oxide by Thenard, is a subsulphate. I placed it on a filter, washed it very carefully, and dissolved it, while still moist, in pure muriatic acid, the solution being effected without difficulty, although the same salt when dry could only be dissolved by long boiling. The caustic ammonia threw down oxide which weighed ·855 gr. after ignition; the muriate of baryta ·945 of sulphate of baryta, weighed with the same precaution, implying ·321 of sulphuric acid. Consequently 100 parts of sulphuric acid were here combined with 266 of oxide of iron, and the *subsalt* consists of

Sulphuric acid	27·33	100
Oxide of iron	72·67	266

Since 100 parts of sulphuric acid neutralise 65·5 of oxide of iron, and in the subsalt take up 266 parts, that is four times as much, it is evident that the same law prevails here as with regard to the subsalts of copper. This gives 22 parts of sulphur for 100 of iron, which indeed is one quarter of the quantity of sulphur contained in the neutral salt of the oxide, but stands in no simple relation to the quantity of sulphur with which iron can be combined without the presence of a third body; a proof that, under certain circumstances, nature departs, in the case of compli-

cated

cated bodies, from the proportions which she observes in simpler combinations. We see here, that although in the neutral salt of the protoxide the proportion of the component parts is determined by the sulphur and the iron, it depends no longer on these elements in the subsalts of the oxide of iron, but on the acid and the base, which unite in such a proportion, that the acid takes up a certain portion of oxygen in the neutral salt, and four times as much in the subsalt: and this irregularity arises from the quantity of oxygen in the oxide, which is not twice as much as in the protoxide, but only once and a half. Hence the affinity of the oxygen for the iron of the oxide forcibly introduces a proportion for the sulphur and the iron of the salts of the oxide, totally different from that which they have originally a tendency to observe. In the salts of the oxide of copper, on the contrary, similar proportions are observed between the sulphur and the copper, because the oxide contains twice as much oxygen as the protoxide. These half steps, or half intervals, as they may be called, in which a subsequent combination contains only half as much more as a preceding one, will require, in future analyses of complicated combinations, a greater readiness in calculation, and a greater precision of conception, than if each step corresponded with a double proportion, or at least with a multiple of some preceding step.

I have already mentioned, that subsulphate of the oxide of iron is deposited by a solution of the neutral sulphate of the protoxide, and that the salt, which then crystallizes, is of an emerald green, and forms oblique rhombs. This salt is a triple combination of the neutral sulphates of the oxide and the protoxide. The yellow colour of the former turns the blueish hue of the latter to a purer green. I have reason to consider this salt as the same which Thenard supposed to be a completely neutral salt of the protoxide. When we dissolve the most regular crystals in water, we obtain a solution slightly greenish, from which a small addition of ammonia throws down a subsulphate of the oxide. This is at its first precipitation white or green, the oxide and the protoxide being separated together; but after some minutes the latter falls no longer, and the precipitate is the yellow subsalt of the oxide, even in close vessels. Exposed to the air, the solution of the salt of the protoxide absorbs oxygen very rapidly, until the triple salt is formed; and if we boil it and cool it in succession for several days together, we obtain a red syrup-like salt, which affords a green

green precipitate upon the addition of caustic ammonia in great abundance. This is Thenard's supersalt of sulphuric acid and black protoxide of iron. If, on the contrary, we add the ammonia by little and little, the salt falls down at first yellow, and afterwards green. This uncrystallizable salt is again a triple combination of the two salts of iron, and it is to be presumed that it contains one of its constituent parts in twice or four times as great a proportion as the emerald green salt. If we boil this salt with nitric acid, it is converted into a salt of the oxide, which, after the expulsion of the nitric acid, leaves a portion of the subsalt undissolved. The solution of the neutral sulphate of the protoxide is orange, but becomes light yellow when it is diluted with water and decomposed by an acid, as the yellow salts of iron lose a great part of their colour by the addition of an excess of acid; but I do not consider this circumstance as a proof of the existence of an acid sulphate of the oxide of iron, as Thenard maintains. Without doubt M. Thenard holds a distinguished rank among the chemists of our time, and his results may be considered as possessing considerable authority. So much the more unfortunate is it when such a person ventures to ground on such experiments, as have certainly been made, and considered as unsatisfactory, by other chemists, a systematic essay, which discourages others from investigating the subject, as they entertain no doubt of the accuracy of the facts that are advanced in it.

Since I have not hitherto been able to discover any supersalt of the sulphuric acid and the protoxide of iron, whatever probability there may be of the existence of such a compound, there is hitherto no known saline compound corresponding to the natural pyrites. May not the non-existence of such a salt be the reason that this pyrites remains so little altered, notwithstanding its exposure to moisture in our mines? So also its insolubility in dilute sulphuric and muriatic acid depends on the inability of the hydrogen of the water to combine with the sulphur in more than one proportion; while the pyrites contains precisely a double portion, as we shall see hereafter.

[To be continued.]

XLVI. *Re-*

XLVI. *Researches upon the Heat developed in Combustion, and in the Condensation of Vapours. Read before the French Institute on the 24th of February and 30th of November 1812. By* Count Rumford, *F.R.S. Lieut.-General in the Service of the King of Bavaria, Foreign Associate of the Imperial Institute of France, &c. &c.**

Attempts have been long made to measure the heat which is developed in the combustion of inflammable substances; but the results of the experiments have been so contradictory, and the methods employed so little calculated to inspire confidence, that it is with reason that the whole labour has been considered as not far advanced.

I have undertaken it thrice within these twenty years, without success. After having made a great number of experiments with the most scrupulous care, and with an apparatus long since contrived, and afterwards prepared by clever workmen; I have nevertheless done nothing worthy of being made public. A large apparatus of copper, upwards of twelve feet long, which I constructed at Munich fifteen years ago, and another one not less expensive, made at Paris four years ago, and which I have still in my laboratory, are testimonies of the desire which I have long entertained to find the means of elucidating a question which has always appeared to me of great importance both with respect to the sciences and the arts.

I have now the satisfaction to announce, that after all my fruitless attempts, I have at length found out a very simple method of measuring the heat which is manifested in combustion, and even with a precision which leaves nothing more to be desired.

In order that a proper estimate may be formed of my method of operating, and of the confidence which may be reposed in the results of my experiments, I have exhibited my apparatus to the class.

The principal part of this apparatus is a kind of prismatic receiver, 8 inches long by $4\frac{1}{2}$ broad, and $4\frac{3}{4}$ inches in height, constructed of very thin leaves of copper. This recipient, which may well merit the already celebrated name of Calorimeter, is furnished with a long neck or gullet near one of its extremities, three quarters of an inch in diameter and three inches high, which is destined to receive, and to keep in its place, a mercurial thermometer of a particular form.

* Translated from a French copy transmitted by the author to Sir Humphry Davy, to whose kindness we are indebted for the communication.—Edit.

form. This receiver has also another gullet of an inch in diameter, and an inch in height, situated at the centre of its upper part, which is closed by a cork stopper.

In the interior of this receiver, two lines from the bottom, is a peculiar kind of worm, which receives all the products from the combustion of the inflammable bodies which are burnt in the experiments, and which transmits the heat evolved in this combustion to a considerable mass of water which is contained in the receiver.

This worm, which is constructed of thin plates of copper, occupies and covers the whole bottom of the receiver, without touching, however, either the sides or bottom. It is a flat tube, an inch and a half broad at one of its extremities, an inch broad at the other, and half an inch in height or thickness throughout. It is folded horizontally, so as to pass thrice from one extremity of the receiver to the other; and it is kept in its place by several small feet, at the height of two lines above the bottom of the receiver.

The aperture which forms the mouth of the worm, is a circular hole in its bottom near its extremity, where it is broadest; and at this hole a vertical tube is soldered, an inch in diameter and an inch in height, which enters a quarter of an inch into the interior of the worm above the level of the bottom.

This tube passes through the bottom of the receiver by a circular hole made for the receiver, and where it is soldered: its lower aperture, which is open, is seven lines below the level of the bottom of the receiver, and it is by this passage that the products of the combustion are made to pass into the worm.

The other extremity of the worm traverses horizontally the vertical side of the extremity of the recipient opposed to that near which the products of the combustion enter the worm.

The worm, before passing through the vertical side of the extremity of the recipient, takes the form of a round tube six lines in diameter, and a piece of this tube, an inch in length, is seen outside of the receiver. This piece is intended to enter another similar tube belonging to the worm of a second receiver, which I call the *secondary receiver*, and which is intended to receive the heat which might still exist in the products of the combustion after they have passed through the worm from the *principal receiver*.

In order to support these two receivers in the air, so as not to touch the table on which they are placed, they are both fixed into squares of dry linden wood, made of sticks

sticks of an inch square each: a border of copper, three lines in breadth, which descends quite round the bottom of the receiver, serves to attach the receiver to its wooden frame by means of a row of very small nails. The body of the receiver enters about one line into its frame, and fills it up exactly.

The perfection of this apparatus depends essentially on the form of the worm, as will be seen when we consider the object for which it is intended.

The products from the combustion being all elastic fluids, and consequently substances which cannot communicate their heat except by proceeding particle by particle to deposit it on the surface of the cold and immoveable body which is destined to receive it, it became indispensable to arrange the apparatus so as to make these warm fluids necessarily pass under and against a large flat surface placed horizontally and always cold.

Previous to making use of horizontal worms constructed of flat pipes, I had tried more than once those of the common form; but they never answered my purpose in a perfect manner, and I never could take any account of the experiments in which they were employed.

There is no doubt that the form which I adopted for the worm of my calorimeter would be very advantageous for all kinds of distilling apparatus.

One thing very important in the arrangement of my apparatus, is the form of the thermometer which I use for measuring the temperature of the water in the receiver. This thermometer, which I made myself ten years ago, and which after undergoing many trials always appeared good, is a mercurial thermometer divided according to the scale of Fahrenheit. It is one of four thermometers, all similar, which I employed in my researches upon the cooling of liquids inclosed in vessels, made at Munich during the winter of 1802–3. Its reservoir, which is cylindrical, is only about two lines in diameter, while it is four inches high; and as the water in the receiver of my thermometer is four inches deep, this thermometer always indicates the mean temperature of this liquid, *whatever may be the temperatures of its different strata*.

I have frequently had occasion, in my different researches upon heat, to notice the importance of this precaution; and I cannot conceive how we may expect to avoid great errors in measuring the temperature of heated or cold liquids, if we neglect to pay attention to them. For my own part, I freely

I freely admit, that I have paid very little respect to some experiments which have been communicated to me, when I knew that they have been performed in so negligent a manner; and certainly I shall never throw away time in endeavouring to build theories on their results.

In using the apparatus which I have described, it is necessary to have recourse to several precautions. It is easy to see in the first place, that when it is necessary to determine the quantity of heat developed in the combustion of any inflammable substance, it is indispensably necessary to arrange matters in such a way as to make *the combustion complete*; and I am of opinion that it may be regarded as complete, when the substance burned no longer leaves any residue, and burns with a bright flame without smoke or smell.

The least smell, particularly that which is peculiar to the inflammable body which we burn, is an infallible indication that the combustion is not perfect.

It was long before I discovered the method of burning in a satisfactory manner the very volatile liquids, such as alcohol and ether; but I finally succeeded, as will presently appear. I have frequently succeeded in burning highly rectified sulphuric ether, without the least smell of ether being perceptible in the room, and it was only under these circumstances that I regarded the experiment as accurate.

As to the woods, I discovered a very simple method of burning them without the least appearance of smoke or smell. I procured from a joiner some chips of wood about six lines broad and one-tenth of a line in thickness; and by holding them between the fingers, or with a pair of pincers, elevated at an angle of 45 degrees, and with their edge in a vertical position, they burned like a taper, and with a very fine flame.

The piece of wood which is burnt being very thin, and being between two flat frames which embrace it very closely, is exposed to the action of so strong a heat that it burns completely. If very thick chips are employed, a part of the charcoal of the wood remains, particularly if it be oak, or any other wood which burns slowly; and in this case the experiments are not good; but in making use of thin well dried chips, I have discovered that all kinds of wood may be burned completely.

In burning candles, tapers, and oils in lamps, the only precautions necessary consist in arranging the wick in such a way as to give out no smoke; then place the flame conveniently under

under the aperture of the worm, and to cover the apparatus on all sides by screens, to prevent the flame from being deranged by the wind.

There is in these experiments a source of errors too evident to escape the most superficial observation, and to which it is indispensable to pay attention. While the calorimeter is heated by the heat developed in the combustion of the inflammable substance which is burnt at the aperture of its worm, it is continually cooled by the circumambient air. It would doubtless be possible, by calculations founded on a knowledge of the law of the cooling of the receiver, which might be discovered by particular experiments, to determine the measure of the effect produced by the cooling in question, and even with a certain degree of precision: but it would be impossible to appreciate by this method, or by any other method known, the effects of another cause of error, less ostensible perhaps, but certainly more powerful, than that of the cooling of the external surface of the receiver.

The azote which is mixed with the oxygen of the atmospheric air is necessarily carried into the worm, with the products properly belonging to combustion; and without a precaution which occurred to me to prevent the effects of *this* cause of error, by compensating for them, all my experiments would have been useless.

Fortunately the method which I employed to prevent the effects of this cause of error, was sufficient to prevent at the same time those which might have resulted from the cooling of the external surface of the receiver.

As the receiver is not cooled by the atmospheric air which touches its external surface, or by the azote and the other gases which pass through the worm with the products of combustion, unless the worm be warmer than the surrounding air; and as on the contrary it is heated by these same elastic fluids, always when its temperature is lower than theirs; by taking care that the temperature of the water in the receiver is always at the commencement of an experiment a certain number of degrees by the thermometer (five for example) below the temperature of the air, and by finishing the experiment at the instant when the water in the receiver shall have acquired a higher temperature than that of the air of the same number of degrees, the receiver will be *heated* by the air during half the time occupied by the experiment, and *cooled* during the other half: the calorific and frigorific effects of the air on the apparatus will be counterbalanced so as not to produce any sensible effect on the results of the experiment, and consequently so as to require no correction.

When experiments are undertaken with a view to elucidate the phænomena of nature, it is always more satisfactory to avoid errors, or to compensate for them, than to rely upon calculations to appreciate their effects.

As the law of the variation of the specific heat of water at different temperatures is not known, and as we are but imperfectly acquainted with the true measurement of the intervals of temperature which are marked by the divisions of our thermometers; in order to prevent the effects of our uncertainty on this head, as to the results of the inquiry in question, I took care to make my experiments in a room in which the temperature varied very little, and to confine them to a very trifling elevation of the temperature of the water in the receiver. It is true that I performed some experiments in a room where the air was much colder, and in which I filled the receiver with ice instead of water; but these experiments were for a particular purpose, and they are not arranged along with the others. Besides, they never yielded results so constant or satisfactory as those of the experiments made under other circumstances.

In order to give an idea of the confidence which may be placed in the results of the experiments made with the new apparatus which I have described, I shall subjoin the details of an experiment made with the express view of discovering the measurement of its perfection.

Having filled two receivers properly attached to each other, with water at the temperature of the air of the room, that of 55° Fahrenheit, I burned a taper under the mouth of the principal receiver, so that all the products of the combustion passed through the worm of the secondary receiver, after having passed through that of the principal. Each of the receivers contained 2371 grammes of water.

The following were the results of this experiment:

Time of the Observation.			Temperature of the Water in the principal Receiver.	Temperature of the Water in the secondary Receiver.
Hours.	Min.	Seconds.		
9	37	0	55° F.	55° F.
0	49	42	65	55
0	56	15	70	55
10	2	52	75	$55\frac{1}{8}$
	9	32	80	$55\frac{3}{8}$
	16	34	85	$55\frac{5}{8}$
	23	54	90	$55\frac{3}{4}$
	27	..	..	56
	31	40	95	$56\frac{1}{8}$
	39	35	100	$56\frac{3}{8}$
	47	40	105	$56\frac{3}{4}$

It

It would seem from the results of this experiment, that the water in the secondary receiver only began to be perceptibly heated after that which had been in the principal receiver had been already heated from 15 to 20°; and as I proposed to myself, from the commencement of this work, never to continue an experiment longer than the temperature of the water in the principal receiver was raised to 10 or 12° F. it may be conceived that as soon as I had learnt by this experiment how much heat remains in the products of the combustion, after they have passed through the worm from the principal receiver, I renounced the project which I had first conceived of working with the two receivers joined together. As it was evident. from the results of this experiment, that the second receiver could never be sensibly affected, or indicate any thing, notwithstanding the confidence which I ought to have in the indications of the first, I have taken the resolution to get rid of it.

We shall see by the description which I have given of this apparatus, that we may make use of it very conveniently in order to determine the specific heat of the gases, as well as that which is manifested in the condensation of vapours, and generally in all the researches for measuring the quantity of heat communicated by a given elastic fluid in its cooling; and as it would be very easy, by a simple process, to separate completely the products of the vapours condensed in the worm, and of the gases which pass through it without being condensed, I do not refrain from hoping that this apparatus will become useful as an instrument to be employed in chemical analyses. Besides, this will be only an extension of the method already employed with so much success by M. Saussure and by Messrs. Gay-Lussac and Thenard.

As soon as my apparatus was finished, I was anxious to ascertain what quantity of heat I should find in the combustion of wax and of olive oil, that I might afterwards compare the results of my experiments with those of M. Lavoisier; and as I have the most implicit confidence in every thing which this most worthy man has published, I was desirous to find in this comparison a proof of the exactitude of my method, and at the same time a confirmation of M. Lavoisier's calculations.

§ I. *Heat developed in the Combustion of Wax.*

The air of the room being at the temperature of 61° F. 2781 grammes of water at the temperature of 66° F. were

 placed

placed in the receiver of the calorimeter (including the quantity of this liquid which represents the specific heat of the instrument); and a lighted taper having been placed at the mouth of the worm, the calorimeter was heated for 13 minutes and 26 seconds. When the thermometer announced that the water had acquired the temperature of 66° F. the taper was extinguished.

As I took care to weigh the taper before lighting it; on weighing it again at the end of the experiment, I found that 1·63 gramme of wax had been burnt.

In order to express the results of this experiment in a way to render them palpable, and at the same time easy to be compared with the results of other similar experiments, we shall show how much water at the freezing point, the heat manifested in the combustion of 1·63 gramme of wax which were burnt, must have boiled under the mean pressure of the atmosphere.

The interval upon the scale of Fahrenheit's thermometer, between the freezing and boiling points, being 180 degrees; if, in order to raise the temperature of the water in the calorimeter 10 degrees, we must burn 1·63 gramme of wax, 29·34 grammes must be burnt in order to raise it 180 degrees: and if 29·34 grammes of wax can furnish a sufficiency of heat in their combustion to raise the temperature of 2781 grammes of water 180 degrees; one gramme of the same flammable substance ought to furnish enough to heat 94·785 grammes of water the same number of degrees.

Consequently, one pound of white wax, or of a taper which we burn, ought to furnish in its combustion enough of heat to raise 94·785 pounds of water from the freezing to the boiling point.

In order to ascertain how many pounds of ice the same quantity of water would be capable of melting, we have only to add to the number of pounds of water at the temperature of ice which this heat is capable of boiling, the third part of this number, and the sum will express the weight in pounds of this quantity of ice*.

For white wax therefore —94·785
+31·595

=126·380 pounds of thawed ice for one pound of the substance burnt.

* It is known that the same quantity of heat which is necessary for melting one pound of ice, would be sufficient for heating and boiling three quarters of a pound of water at the temperature of ice.

Before

Before comparing the result of this experiment with that of an experiment made with the same substance by M. Lavoisier, I shall give an account of two other experiments made by myself with wax, and my readers will no doubt be struck with the uniformity of their results. It is in fact so remarkable, that I should scarcely dare to publish them, if I had not proofs that all my experiments were really made and registered before commencing any calculation on their results, and if I was not certain that those who prefer adopting my method, by using the same apparatus, will have the same results, if they repeat my experiments.

As the method of proceeding in making these experiments ought to be now well known, I can without inconvenience suppress the details, and give only the results of the experiments.

I shall begin with three experiments made with bees wax; and in order to render them easy to be compared, I shall present them together in a table.

Experiments made with Bees Wax.

No. of Experiments.	Quantity of Wax burnt.	Time employed in the Combustion.	Quantity of Water heated.	Elevation of its Temperature.	Temperature of the Water. At the commencement of the Exp.	Temperature of the Water. At the end of the Experiment.	Temperature of the Air.	Results. Pounds of Water heated to 180° F.	Results. Pounds of Ice dissolved.
	gram.	m. sec.	gram.					liv.	liv.
1	1·63	13·24	2781	10° F.	56°	66°	61°	94·765	126·38
2	2·36	19·30	. .	$14\frac{1}{2}$°	51°	$65\frac{1}{2}$°	58°	94·926	126·608
3	2·17	28·15	. .	$13\frac{1}{4}$°	$51\frac{3}{4}$°	65°	58°	94·337	125·783

If we take the mean term between the results of these three experiments, we shall find that the quantity of heat developed in the combustion of wax is such, that a pound of this substance is sufficient to heat and boil 94·682 pounds of water at the freezing point: consequently one pound of wax ought to be sufficient, when burnt, to melt 126·242 pounds of ice.

In the experiments of M. Lavoisier, the heat developed in the combustion of a pound of bees wax was sufficient to melt 133·166 pounds of ice.

The difference between the results of our experiments made with this substance is not very great; and if those of M. Lavoisier were made at a time when the temperature

 of

of the air was only a few degrees higher than that of ice (a circumstance with which I cannot be acquainted), the quantity of azote which must have entered into the calorimeter with the oxygen employed to keep up the combustion, is so great, that it would be sufficient to account for the difference; but the very great difference which exists between the results of our experiments made with oil of olives, proves that one or other of our processes must have been faulty.

§ II. *Heat developed in the Combustion of Olive Oil.*

The mean result of several experiments made with oil of olives gave me as the measure of the quantity of heat developed in the combustion of one pound of this substance, 90·439 pounds of water heated 180° of Fahrenheit, or 120 pounds of dissolved ice, omitting fractions.

In the experiments of M. Lavoisier there were upwards of 148 pounds of ice dissolved by the heat which appeared to result from the combustion of one pound of this oil. It is true that this excellent experimenter has himself regarded this result as too great, and he adds with becoming modesty, "We shall probably be obliged to make considerable corrections in most of the results which I have given; but I did not cosider this a good reason for delaying to assist those who may purpose to engage in similar inquiries."

§ III. *Heat developed in the Combustion of purified Oil of Colsa, such as that which is sold in Paris for Lamp Oil.*

As it appears very probable that all the fat oils which are perfectly pure, are composed of the same elements, I was anxious to know if oil of colsa, purified with sulphuric acid, would not give out more heat in its combustion than oil of olives when burnt in its natural state. The results of three experiments made with purified oil of colsa, convinced me that in fact this oil gives more heat in its combustion than olive oil: the difference is even considerable, and more than I had suspected.

With the combustion of one pound of refined oil of colsa . . . . 93·073 pounds of water heated 180°,
one pound of olive oil 90·439 ditto ditto.

Chemists are best qualified to inform us if the quantity of *incombustible* matter separated from the oil of colsa, in refining it, be sufficient to account for this difference.

On comparing the results of the experiments made with bees wax with those of purified oil, it seems that, the weight being equal, these two substances furnish in their combustion

tion nearly equal quantities of heat; and as this ought to be the case, in fact, according to the quantities of combustible matter which these substances contain, this result is made in order to give confidence to the method of measuring the heat which is developed in combustion.

With the combustion of one pound of bees wax 91·632 pounds of water were heated 100°, and with one pound of refined oil 93·073 pounds of water were heated 180°.

As the object which I had chiefly in view in these experiments was to determine the quantities of heat which are developed in the combustion of pure hydrogen and carbon; in order to render this new method useful in chemical analyses, I attached myself particularly to inflammable substances which have been analysed with the greatest care.

§ IV. *Estimate of the Quantities of Heat developed in the Combustion of Hydrogen Gas and Carbon.*

Several attempts have been made to determine these interesting questions by direct experiments, by burning *pure hydrogen,* or hydrogen gas, and *pure charcoal;* but the results of these inquiries have been so variable that we cannot rely upon them.

According to Crawford, the heat developed in the combustion of one pound of hydrogen gas is sufficient to raise the temperature of 410 pounds of water to 180° F. but the estimate of M. Lavoisier is much lower: according to him, this heat could raise only 221·69 pounds of water to this heat.

In return, M. Lavoisier estimates the quantity of heat developed in the combustion of charcoal much higher than Mr. Crawford. I have a great many reasons for thinking that both estimate too highly; and if this opinion be confirmed, we shall be obliged to estimate the heat developed in the combustion of hydrogen even a little higher than Mr. Crawford, in order to account for that which was manifested in my experiments.

According to the results of several experiments made five years ago, it appeared to me that the heat developed in the combustion of one pound of charcoal, dried as well as possible before being weighed, by making it red hot in a crucible, was not fit to raise more than 52 or 54 pounds of water from the freezing to the boiling point.

According to Crawford, this heat ought to be sufficient to boil 57·606 pounds, and according to M. Lavoisier 72·475 pounds.

We shall see how these estimates agree with the results of my experiments.

As the experiments made with *wax* have given very uniform results, and as the analysis of this substance has been effected with great care, I shall show how the quantities of hydrogen and carbon which are found in this substance agree with the quantity of heat which it furnished me in combustion.

According to the analysis of Gay-Lussac and Thenard, one pound of this substance contains

Carbon........ 0·8179 } pound.
Free hydrogen.. 0·1191 }

If we adopt the calculation of Mr. Crawford both as to the heat furnished by the hydrogen, and that furnished by the carbon, we shall have:

	Pounds of Water boiled.
For the heat which ought to be furnished by 0·8179 pound of charcoal, at the rate of 57·606 pounds of water at the freezing point made to boil, per pound of charcoal burnt..................	47·116
For the heat which ought to be furnished in the combustion of 0·1191 pound of hydrogen, at the rate of 410 pounds of water boiled, per pound of hydrogen burnt.........................	48·831
Total heat which ought to be furnished by the quantities of combustible matters (carbon and hydrogen) which are found in one pound of bees wax..................................	95·947
Quantity of heat furnished by one pound of bees wax in its combustion, according to my experiments..................................	94·662

If we adopt the calculations of M. Lavoisier, for the heat furnished by the carbon and hydrogen in their combustion, we shall have:

For the heat which ought to be furnished by 0·8179 pound of charcoal at the rate of 72·375 pounds of water heated 180° per pound........	59·059
For that which ought to be furnished by 0·1191 pound of hydrogen at the rate of 221·64 pounds water 180° per pound......................	26·403
Total heat which ought to be furnished by the combustible matter in one pound of bees wax ...	85·462

From the results of these calculations, it will be seen that those of Mr. Crawford agree much better with my experiments than those of M. Lavoisier.

Let us now see how the results of the experiments made with the fat oils agree with the estimates of Messrs. Lavoisier and Crawford. Ac-

According to the analysis of Messrs. Gay-Lussac and Thenard, one pound of oil of olives ought to contain:

Of carbon	0·7721 pound.
hydrogen at liberty ..	0·1208 pound.

According to the calculation of M. Lavoisier,

	Pounds of Water heated 180°.
For the 0·7721 pound carbon	55·881 lbs.
and for the 0·1208 pound hydrogen	26·78 lbs.
Total	82·661

And according to the calculations of Mr. Crawford,

For the 0·7721 pound carbon	44·478 lbs.
and for the 0·1208 pound of hydrogen	49·528 lbs.
Total	94·006 lbs.

According to my experiments, one pound of refined oil of colsa has furnished a sufficiency of heat to raise 93·073 pounds of water to 180°, and one pound of oil of olives sufficiently to heat 90·439 pounds of the same water.

It results from all these comparisons, that the estimates of Mr. Crawford agree much better with the results of my experiments than those of M. Lavoisier.

[To be continued.]

XLVII. *An Account relative to the Situation of His Majesty's late Ship Royal George, sunk at Spithead in the Year 1782; together with the Value, and Means of raising her.* *By Mr.* J. Hicks, *of No.* 22, *Charlotte Street, Rathbone Place.*

It has been conjectured by many, that the Royal George must be nearly covered with sand and mud, and that any attempt to raise her would be fruitless; and also, that in the event of her being raised, the hull and her stores must be so decayed and injured by the length of time she has been sunk, that the produce would not pay for the trouble and expense; which has induced me, at the request of many friends, to give an account of her situation and value, with the expenses of raising her.

After showing my plan, model, &c. to the Lords of the Admiralty, I received an order on the 6th of August last, informing me their Lordships had caused directions to be given to Admiral Sir Richard Bickerton, Bart. at Portsmouth, to call together a Committee of the most scientific Captains who might be at that port, with the Commissioner, the Master

Master Attendant, and the Master Builder of the Dock Yard, to inquire into and report upon the probability of success attending my plan, and for me to proceed to Portsmouth with my model, &c. for the consideration of the Committee, and to take soundings upon her, &c. which was accordingly done by me and two of His Majesty's pilots (Mr. J. Paddon and Mr. R. Hartfield), with poles, leads, and lines, in September last. The result proved that no deviation had taken place since I took soundings first (which was two years after she went down). The report from the Committee to their Lordships proved so favourable and satisfactory, that they were pleased to obtain an Order in Council, that my proposal should be acceded to; namely, that I should be allowed to raise the Royal George at my own expense, and receive as my reward, should I succeed, the ship with all her stores, guns, and other articles contained in her.

The Royal George lies in the middle of the best anchorage at Spithead, in thirteen fathoms at low water, and on a bed of stiff blue clay, in which she has not sunk seven feet, the bed of the sea close along side and for a considerable distance about her a perfect level, no sand, nor any other obstruction further than the natural sediment of water, of which there can be but little, as the pilots and myself could distinctly hear the poles and leads strike the decks.

References to the Plate. (Plate VIII.)

A. The Royal George, showing the position she now lies in.

B. Shows a frame-work and stage, to be put down on the bed of the sea, close to each side, and on the deck of the sunk ship, fore and aft, by which means the diving machine, bell, and the purchases, can be lowered to any part with exactness and nicety, which could not be accomplished in any other way.

C. Represents a temporary house for the men, which will save much time and the expense of a vessel.

D d. Trusses, of which there are to be fourteen, supported by the frame-work, with the upper blocks, till the whole are ready for the lifting ships.

E e. The upper block, with a reaving fall.

F. The clasp purchases in the lower deck port-holes, forming two treble blocks each, with seven-inch rope, which will lift upwards of 5000 tons.

G. Front view of the clasp purchase made of cast iron, three feet three inches by two feet eight inches.

H. Side view of ditto.

I. A

I. A claw hook (with a screw and horned nut) to be turned up when the clasp purchase is in the port-hole by a man in the diving machine, which prevents the possibility of its falling out.

K. Lifting ships.
L. Low water line.
M. Bed of the sea.
N. Gunwale of the lifting ship.

Estimate of the Value of the Ship, Stores, &c. and of the Expense of raising her: taken from good Authority.

Estimate of Value.

	No.		£.	s.	d.
Guns, iron, 32 pounders	28	£1540			
12	10	.. 300			
£1840 at ....£1 0 0 per cwt.			1,840	0	0
Brass, 24 dolphins and chased	28	..1484			
12	19	.. 589			
£2073 at ....£10 0 0 per cwt.			20,730	0	0
Round shot 32		1680			
24		1960	£1230		
12		3080			
at 12*s.* 6*d.* per cwt.			768	15	0
	Tons.				
Iron ballast	200	at £5 0 0 per ton	1000	0	0
Coals	50	at 2 5 0 ———	112	10	0
Cables and cordage*	60	at 70 0 0 ———	4,200	0	0
Sails			500	0	0
Copper, &c. on her bottom, cost 2,050*l.*; worth above three-fourths as old copper			1,537	0	0
Beef, 11 tons; pork, 13 tons; butter, 2 tons; pease, 13 tons; flower, 8½ tons; powder, 11 tons; spirits, wine, vinegar, and water.					
The casks that contain the above, copper hoops, &c			500	0	0
Cash, and other valuables			5,000	0	0
Hull, 2,046 tons burthen, worth 80,000*l.* when lost, one quarter is			20,000	0	0
			£56,188	5	0

* I have good authority for putting this value on the cables and cordage (which is upwards of 20*l.* per ton lower than the present price), as instances are known of cables being under water fifty years without injury.

Estimate

Estimate of Expenses.

				£	s.	d.
Cast iron queen posts, for trusses				£224	0	0
Shoes for sounding poles				14	8	0
Clasp purchases, with theaves and pins				896	0	0
Diving machines, bell, ballast, and frames				556	0	0
Two cast iron winches (shifting power)				160	0	0
Twenty-four beds ... 25*s.*	£30	0	0			
Divers' dresses	5	17	0			
Firehearth	25	0	0			
Stove	5	0	0			
Camp forge (complete)	25	0	0			
Tools, &c.	60	0	0			
				150	17	0
Two boats to attend the work				10	0	0
Wages and victualling 24 men, eight weeks				565	12	0
Workmanship on timber for trusses, poles, and frame	£283	10	0			
Iron for ditto	352	0	0			
Extra sawing, halling timber, coals, lights, &c.	£187	13	0			
				823	3	0
Spare cash for incidental expenses				600	0	0
Security to Government (or bondsmen to the amount)				6,000	0	0
				£10,000	0	0

	Value.			Loss.						
Timber for trusses, and frame	£3,888	0	0	$\frac{1}{10}$	£388	16	0			
Iron for ditto	606	0	0	$\frac{1}{8}$	76	0	0			
Spars for sounding poles	21	0	0	$\frac{1}{7}$	3	0	0			
Cordage, 673 cwt. at 97*l.* per ton	3,312	0	0	$\frac{1}{8}$	414	2	0			
Boat hire				all	10	0	0			
Wages and victualling				all	565	12	0			
Workmanship on timber, iron, &c. &c.				all	823	3	0			
Clasp purchases, about					210	0	0			
								2,490	13	0

☞ Any person wishing to become a subscriber, and conceiving £50,000 above the value of the ship and all that is in her, I will agree to forfeit half my share, in the proportion to the number of shares they may subscribe, if they will in like manner forfeit all their advantage in their shares above £50,000.

J. Hicks.

Mr. Hicks has issued the following Proposals for raising by loan the sum of ten thousand pounds, in one hundred shares of one hundred pounds each: to be applied to the purpose of raising His Majesty's ship Royal George, sunk at Spithead; which, upon the representation and recommendation of the Lords of the Admiralty, His Royal Highness the Prince Regent has been graciously pleased, in the name and on the behalf of His Majesty, and by and with the advice of His Majesty's most honourable Privy Council, to give to Mr. James Hicks (late Secretary to the Hon. Sir Henry Edwin Stanhope, Bart. Admiral of the Blue), the hull, furniture, naval, victualling and ordnance stores of the said ship (which is copper bottomed), estimated at upwards of Fifty Thousand Pounds!

The plan has been seen and approved by most of the superior mechanics in the kingdom.

It is proposed in the event of the ship being raised, (of which there is not the least doubt,) that the subscribers shall be repaid their subscription money out of the first property recovered: and half the remaining sum to be equally divided among the subscribers, immediately after the sale of the ship, furniture, naval, victualling, ordnance stores, and the materials, in proportion to the number of shares subscribed.

The Lords of the Admiralty have kindly consented to furnish Mr. Hicks with all the timber, iron, ropes, blocks, mooring anchors, cables, bridles, buoys, &c. equal in value to £18,000, together with the lifting ships and as many men and officers as will be requisite; and for which their Lordships only require £6000 to be deposited in their hands, to cover any loss that may arise by conversion, wear and tear, &c.; (which, upon a fair calculation, cannot exceed one-fourth of the whole amount of the subscription;) of which sum Government require only £1000 to be deposited upon Mr. Hicks's commencing his operation.

The Lords of the Admiralty have agreed to take back all the stores, which may be issued to Mr. Hicks, at a fair valuation after they shall be done with.

Conditions.

The subscribers to pay the deposits on their respective shares into the hands of Messrs. Smith, Payne, and Smith, and Messrs. Hammersley, bankers, as follows:

At the time of subscribing, or when the subscription is full	£25	0	0	on each share.
On the 24th May	10	0	0	

On

On the 14th June £50 0 0
On the 1st July 15 0 0

But it is presumed that the whole of the subscription will not be called for, and a committee will be formed from among the subscribers, to see the due application of the money.

N. B. Half shares will be admitted.

Particulars of an estimate of expenses, together with a copy of the Order in Council, may be seen at each of the said bankers; also at Mr. Hicks's, 22, Charlotte Street, Fitzroy Square, where the model and plans may be inspected, from eleven till five every day, except Saturdays and Sundays.

XLVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

April 1. THE Right Rev. Dr. Goodenough, Lord Bishop of Carlisle, in the chair. Sir Everard Home communicated some additions and corrections to his paper on the Narval. Since writing the first part of it, he has had an opportunity of examining the heads of both male and female narvals; he found in the male a tusk of four feet long, and what he calls a milk tooth imbedded in the substance of the skull about nine inches long; and in a young female, two milk teeth eight inches long likewise imbedded in the skull: hence he concludes that the latter has two tusks of an equal length, and that the former has a tusk and a milk tooth. The tusks of the narval are hollow towards the point, and solid where they are united by a process to the skull.

Dr. C. Wells communicated an account of Harriot Trest, a woman who has her left shoulder, arm and hand as black as the blackest African, while all the rest of her skin is very white. She is a native of Sussex; and the account she gives is, that her mother set her foot on a lobster during her pregnancy. Dr. W. describes the appearance of her skin, her blue eyes, and general comeliness, with much minuteness, as she was a patient in the hospital to which he is physician. He hence infers that blackness of skin is no proof of difference of species, and alleges that the sun does not blacken but rather whitens the skin.

April 8. Lord Morton in the chair. Conclusion of Dr. Wells's paper. The doctor indulged a variety of speculations; supposed with Volney, that the Egyptians were negroes;

negroes; conceived that the want of civilization contributes to make the people black; and referred to various South-sea islanders and others, to sanction this singular fancy. The length of these conjectures prevented the reading of a valuable paper by Professor Berzelius and Dr. Marcet, which was in consequence postponed till the next meeting of the Society after the holidays, on the 29th of April.

GEOLOGICAL SOCIETY.

April 2d, 1813. W. H. Pepys, Esq. Treasurer, in the chair.

The reading of a memoir by Mr. John Farey, Sen. on the Ashover denudation in the county of Derby, was begun. The first part of this paper consists of minute local observations, incapable of abridgement, relative to the inosculation ridges, the basset ridges, the partial incurvation of the beds, and the ascertained or supposed faults.

April 23d. The President in the chair.

Thomas Gregory, Esq. of Bayswater—Thomas Botfield, Esq. of Hopton Court near Bewdley—were severally elected members of the Society.

A notice by the Rev. J. J. Conybeare, M. G. S. relative to the slate of Tintagel in Cornwall, was read, and thanks were voted for the same.

The slate quarries of Tintagel are situated close to the sea, about six miles N. W. of Camelford: they are worked on a large scale, and are celebrated for the excellent quality of the roofing slate which they afford. No dykes of granite or of porphyry have been observed in this rock; but there are veins which afford quartz, rock-crystal of great transparency and beauty, calcareous spar, chlorite, and in some instances adularia. The slate of Tintagel appears to bear a near resemblance to that of Snowdon, and like it occasionally presents the impressions of bivalve shells.

The reading of Mr. Farey's paper on the Ashover denudation was concluded, and thanks were voted for the same.

This portion of Mr. Farey's paper contains a detailed account of the several strata represented in the maps and sections, beginning from the lowest of those that are known.

The fundamental rock of Derbyshire is the fourth limestone. It is supposed to lie at the depth of about 350 yards below the level of the river Amber in Ashover valley. It rises towards the surface under Matlock valley, and actually bassets in Griff-dale. The thickness of this bed is unknown; but as the deep vale of the Dove is entirely excavated in it, without discovering the bottom of the bed, its thickness

thickness cannot be less than 350 or 400 yards. It is generally a pure calcareous freestone, of a whitish yellow colour, disposed regularly in very numerous strata. These consist either of very white marble, or of aggregations of small rhombic crystals: towards the top it is very compact and porcellanous. Few of the beds are without organic remains: in some are found small anomiæ, in others entrochi, or turbinated shells, cornua ammonis, nautili, and branching coralloids. This rock is superficially cracked, so as to present a columnar appearance. Beneath it is much rent, and abounds in shake-holes and large caverns with water-swallows. Some of the fissures connected with the surface are filled with clay sand and quartz pebbles. The veins are filled principally with calcareous spar, heavy spar, and fluor spar: they also contain in their upper part galena, calamine, manganese, red iron ore, white china clay, and steatite.

Upon the fourth limestone lies the third toadstone. The most eastern basset of this rock is at Bonsall upper town. Its thickness in different parts is very various, from four feet to eighty yards. Its usual appearance is that of a cavernous stony mass, of a dirty purplish brown hue. Often it is of a dark blue colour with shining specks, as hard and sonorous as cast iron, also of a light green or blueish gray colour, and rarely it appears as a gritty yellowish stone called Dunstone. Its structure when recent is amygdaloidal, the cavities being filled with green or white globules of calcareous spar. The veins in the limestone above and below this stratum have rarely if ever broken through it; but rents proceed from these into the top and bottom of the toadstone, in which galena and the usual veinstones are sometimes found.

Third limestone.—The most eastern basset of this rock in the line of section is on the western slope of Masson Tor. Its average thickness near Matlock is about 80 yards. Its colour varies from gray to brownish black. It includes several beds of limestone, with layers of dark gray nodular chert. Its organic remains are numerous, and it abounds in mineral veins that afford galena, calamine, and blende, embedded in calcareous spar and heavy spar.

Second toadstone.—The most eastern basset of this rock is in the bed of the Derwent. Its average thickness is greater than that of the third toadstone, and it does not appear liable to such variations of thickness as that rock. In external characters it does not greatly differ from it, except that it contains narrow veins of fibrous calcareous spar.

Second

Second limestone.—The most eastern basset of this rock is in Matlock high Tor. Its average thickness is about 80 yards. Its colour is yellowish or blueish gray. Some of its beds are magnesian limestone. Its principal organic remains are entrochi. It contains metallic veins of galena, calamine, and, as it is said, of copper.

First toadstone.—The first regular basset of this rock appears to be in Matlock high Tor. Its average thickness is about 28 yards. Its general characters differ little from those of the third toadstone, except that it seems disposed in more regular beds or strata.

First limestone.—The average thickness of this rock is 60 yards. Its usual colour is lightish gray: near the top it incloses beds of swinestone interlaid with dark or striped chert. The organic remains of this rock are anomiæ, entrochi, nautili, and other shells, together with many coralloids. It abounds in caverns and water-swallows, and in numerous metallic rake veins, or long vertical rents. Massive fluor (blue John) and elastic bitumen occur in this rock.

The great or limestone shale.—The average thickness of this rock is about 150 yards: its general character is that of a black or dark brown shale, inclosing beds of a soft yellowish sandstone, and of a dark blue limestone; also thin beds of clay, ironstone and septaria. Its organic remains are not numerous, consisting chiefly of anomiæ, myæ, helices, and a few vegetable impressions.

First or millstone grit.—The average thickness of this rock is about 140 yards. It is generally a white or yellowish coarse-grained freestone in thick beds: but at the upper part of the rock is a considerable thickness of soft micaceous thin beds. Its organic remains are large reeds and flags, and occasionally coralloids of a horn-like appearance.

Coal formation.—This lies on the millstone grit, and consists of eighteen beds of grit and of shale; the aggregate thickness of which is 706 yards, and presents the usual characters of the independent coal formation.

PHILOSOPHICAL SOCIETY OF LONDON.

In the course of last month the president, Dr. Lettsom, delivered a lecture on Intemperate Drinking.

Dr. Lettsom commenced by observing, that one of the earliest objects of discovery, nearly coeval with the first history of man, and the knowledge of it, preserved to the present time, is that of intoxication, or the improper use

 of

of things which tend to excite inebriation. After pointing out the knowledge of metals, and the manner of working them, evidenced by Tubal Cain, the lecturer said, that although metallurgy had been brought to a considerable degree of perfection before the deluge, it might be presumed from the silence of the sacred historian, that fermentation, so essential to the production of inebriating liquors, had not been discovered. Noah, who planted the vine in the valley of Mount Ararat, (Baris or Luban, described by Tournefort, who visited it, as the finest valley in Persia,) appears to have been ignorant of the effects of the fermented juice of the grape, till he had experienced its inebriating influence. The memory of the great patriarch was preserved by various rites, till at length the knowledge of their origin was lost, for which those rites had been instituted, probably hastened by the confusion of tongues, and emigration and dispersion which ensued, about 600 years after the patriarch had settled in the valley of Ararat. In these ceremonies they religiously preserved the number nine, constituting the eight persons saved in the ark, with the dove, the messenger of the cessation of the flood. These ceremonies were conducted with secrecy and awful pomp, as early as 1400 years before the Christian æra, particularly at Eleusis; ceremonies strongly resembling those of free masonry in all the lodges in Europe. They were introduced into Europe as early as the Crusades in the 11th century, and the same mystic numbers continued, as nine, or three times three, the root of nine.

On the origin of drinking healths, the learned lecturer related the manner of that of Rowena, the daughter or niece of Hengist, to Vortigern, king of the Britons.—"She came into the room where the king and his guests were sitting. Making a low obedience to him, she said, 'Be of good health, lord king!' Then having drunk, she presented the cup on her knees to the king, who replied, 'I drink your health,' and drank also." This is said to be the origin of the practice of drinking healths; but the President observed, that it was certainly in use as early as the time of Homer, and from the account in Jamblichus, in the Eleusinian or Masonic ceremonies also, accompanied with libations of wine to the mystic number nine. Man, endowed with sentient organs to receive and convey impressions; with intellectual faculties to analyse and modify perception; with powers to evolve thought and constitute mind; and upon whom are conferred the possession and government of the world, enters into his wide domain more helpless and weak than

than the animals destined to his use, or subservient to his subsistence; for, as the impulse of instinct is sooner excited, than the evolution of reason is matured, their wants are supplied with little care, and less reflection, and where animal gratification alone constitutes the summit of happiness. But with man, impressions are as varied and numerous as the objects of creation, and the operations of mind co-extensive with nature, which subject him to feelings of pleasure or pain, of gratification or dislike;—his passions hence become hasty and violent, or slow and temperate; and whilst the latter conduce to intellectual character and dignity, the former sink him to sensual and animal depravity.

The human mind is an existing something, that revolts at quiescence; this spring for exertion is often a source of intellectual improvement, by exciting its energies, till mind itself, as it were, creates mind and action; bold and vehement figures spring up, which wing the thoughts with fire; an animated expression of sentiment, which pervades the whole frame, as blood runs through the veins. But if, from the want of stimulus, this action of the mind is suspended, or weakened, some excitement is sought for, to obviate the horrors of this suspension of the mental functions; and no excitement is so generally grateful, as liquors that have acquired more or less potency by the process of fermentation.

After having shown that where a succession of various functions and amusements keeps the mind in continual occupation, the desire for strong drink is regulated in a great measure by the climate, and diminishes or increases according to the variation of the temperature, the lecturer proceeded to observe, that in Europe and other civilized regions fermented liquors are principally produced from sugar, grape, or grain, and the quantity of spirit made in this kingdom alone amounts to 80,000 tons, which produce a revenue of four millions annually, and destruction to health, happiness, and morals.

The accretion and nutrition of the body is principally produced by the solids taken into the stomach. To divide, dilute, and assimilate these, fluids are requisite as menstrua, and the thinner and purer they are, the better they are adapted to these purposes; and hence the lecturer concluded that water must appear the most prominent, agreeably to the poet of health*. Next to simple water, cyder, beer, and those fluids which contain the least alcohol, may be ranged;

* V. de Armstrong, book I.

and wines, of course, are more safely admissible than spirits, either in their pure or diluted states: all these fermented liquors, however, contain more or less alcohol, and in this proportion are they more or less safe or injurious*. The learned doctor here presented a moral and physical thermometer, or a scale of the progress of temperance and intemperance produced by different liquors; and, having submitted some conjectures respecting the action of spirits on the stomach productive of intoxication, proceeded to the consideration of some of the unhappy effects of intemperance :—loss of tone of the stomach and its digestive powers —disorganization of the functions which prepare, assimilate, and convey the animal juices for the nourishment and health of the body—hard, scirrhous, enlarged and ulcerated liver, jaundice, dropsy, &c. &c.

[To be continued.]

EDINBURGH INSTITUTE.

At a general meeting on Wednesday, the 14th of April, the following among other communications were received.

I. *Account of a new Gun-lock and Breech, invented by Mr. James Thomson, Gun-maker, Parliament Square.*

Mr.Thomson exhibited to the meeting a beautiful fowling-piece, with his improvement, which is applicable to fire-arms of all kinds, and consists of a circular pan, with a cylinder closely fitted to the outside. This external cylinder serves to cut off any superfluous powder, by which means the pan can never be over-primed. It also keeps it perfectly water proof; and the powder being left quite loose, it never fails to explode. This improvement appears to be well worth the attention of Government, as it ensures the action of every musket, however far troops may be required to march in the midst of dew or heavy rain.

II. *Description of a Galvanic Battery, on an improved Construction, invented by Mr. Jackson; communicated by James Millar, M.D.*

Those who have been engaged in Galvanic experiments are well aware of the inconveniences that arise from the loss of time, the great attention requisite, and the considerable expense which is incurred, even when they are conducted on no very extended scale; and brilliant and rapid as the progress of Galvanism has been, it is perhaps owing to such circumstances that the number of those who have

* By recent experiments, Mr. Brande has estimated different fluids at certain degrees. Vide Phil. Trans. for 1811.

been

been occupied in investigations of this nature has been chiefly limited to professional inquirers.

The apparatus of which I am now to give a short description appears to me to be so simple in its construction, and so easy in its application, that it may, in a great measure, obviate these inconveniences. It is the invention of Mr. Jackson, my assistant, who has long employed it for the purpose of applying Galvanism medically. The pairs of plates of which this battery is composed are carefully fused together so as to form one solid mass of metal, and are therefore united in every point of their surfaces. This last circumstance is generally supposed to add greatly to the strength of the battery, and perhaps it serves to increase the power of the battery of Mr. Jackson's construction. The pairs of plates thus prepared are arranged horizontally and alternately with pieces of cloth moistened with the chemical solution employed. A frame, which is also of very simple construction, is all the apparatus necessary to complete the battery. This frame, whose length is to be accommodated to the number of plates employed, has two glass rods at the bottom, as an insulated support to the plates. At each end of the frame there is an upright pillar, through which passes a wire or small bar, which moves horizontally, and is secured by a screw in the top of the pillar, when the bar presses on the plates, to keep them in a vertical position, and in close contact with the intervening moistened cloths. The conducting wires are applied to the poles of the battery in the usual way.

The advantages of this battery over any other that has yet been contrived, will be sufficiently obvious to those who are much conversant with such pursuits; it seems, indeed, to unite the advantages of the simple construction of the pile with the increased power obtained from the trough, but is free from the unavoidable expense which attends the operation of the latter.

1. The first obvious advantage of a battery of this construction is, that it is more portable than any form of the Galvanic trough, whether the pairs of plates in the trough be soldered together and cemented in it, or whether they be moveable, according to the principle of the *couronne de tasses.*

2. Another obvious advantage of this apparatus is, that the original expense is far inferior to that of those constructed in any other form. The materials of which the plates are composed, and the labour of soldering each pair, constitute almost its only expense. A frame of the simplest con-

 struction

struction answers the purpose; and, with a little ingenious contrivance, a temporary frame may scarcely ever be wanting.

3. It is unnecessary to mention to those acquainted with Galvanic apparatus, that it is of great importance to keep the surface of the plates clean, so that the chemical solution may act on the metallic matter to produce the effect. The apparatus before us possesses this advantage in a high degree; because the plates, being in detached pairs, can be easily cleaned, so as always to exhibit, at the commencement of every new operation, all their metallic brightness.

4. But it is one of the peculiar advantages of this apparatus, that its operation is attended with scarcely any expense. It has been one of the great objections to the use of the Galvanic battery, in the form of the trough, that the quantity of nitric acid, to bring a powerful apparatus into full action, renders the expense enormous; for it is found that the effects of the cheaper acids are too rapid and violent: but in this apparatus the cheaper acids, as the muriatic or sulphuric, can be conveniently employed, since the violence of its action is moderated by the mode of its application, through the intervention of the pieces of cloth; and yet the power of its action seems to be greater than batteries of a different construction having the same number of plates in the series, and the same extent of surface.

The apparatus now described has been found to be extremely convenient in the application of Galvanism to medical purposes. For these purposes, I believe, Mr. Jackson first thought of it, and has long and pretty extensively employed it in this way; and 40 pairs of two inches square form a battery of sufficient power for this purpose.

But batteries of a greater number of series, and of greater extent of surface, have been constructed by Mr. Jackson on the same plan; a battery, consisting of 100 pairs of plates of four inches square, was fitted up; and when the cloths were moistened with sulphuric acid diluted with water, the power of action of this battery seemed to be superior to that of the trough, composed of an equal number and of an equal surface of pairs of plates; I say seemed, for no comparative experiments have been yet instituted to ascertain this point, on which I expected to have been able to lay some observations before the Institute at this meeting; but as the experiments are not yet completed, I must reserve them to a future communication. Here, too, it may be added, that a battery constructed of 100 pairs of plates of

six

six inches square, while in action by means of diluted sulphuric acid, produces so powerful an effect as to be able to deflagrate 18 inches of platina wire; an effect which has rarely been exhibited by the trough, even with plates of larger surface, and an equal number of pairs. It may be just mentioned as one reason of the superior action of the battery of Mr. Jackson's construction, that the chemical agent employed in it, namely, the sulphuric acid, could not be used in the trough, even when very largely diluted, without producing so violent an action as to injure the apparatus, and to render its operation extremely incommodious.

As connected with the subject of Galvanism, the brief account of an experiment which shows to what distance the Galvanic fluid may be conveyed through water, will not, I trust, be deemed out of place. The experiment alluded to was first suggested by Mr. Jackson, and was made by him, some years ago, in presence of several gentlemen belonging to the University. For the purpose of having the fact fully verified, the same experiment was repeated yesterday, with his portable apparatus of 40 pairs of two inches. The battery was placed on a rock in the bed of the water of Leith, and a wire from one end was introduced into the river, a few feet from the apparatus: a wire attached to the other end of the battery, and extending 60 yards in length, was carried along the dry bank: the end of this wire was taken in one hand moistened with water, and the other hand was dipped into the river; and although the circuit thus formed was equal to 120 yards, or 360 feet, yet the shock from so small an apparatus was quite perceptible. It was still more sensible when the hand was dipped in the river, and the tongue was applied to the wire. The decomposition of water proceeded rapidly; but the wind prevented a fair trial of the deflagration of metals, and some other experiments which were proposed. In a former experiment, Mr. Jackson found that metals were revived from their solution in acids by the same apparatus, and when the extent of the circuit was not much less. In the course of these experiments, I had an opportunity of witnessing the remarkable effects of the conducting power of bodies, which, indeed, I ought to notice, was observed by Mr. Jackson at the time he made the first experiment. When the wire from one end of the battery was brought into contact with the tongue, at the distance of several yards from it, I perceived a strong metallic taste, or rather

 received

received a distinct shock: at this time I stood on the stony bank, in the bed of the river, and the apparatus was placed on the rock, so that the Galvanic fluid was conveyed through the rock, and the moist earth and stones on the bank.

KIRWANIAN SOCIETY OF DUBLIN.

March 10. A paper "On certain Combinations of the Oxymuriatic Acid, with Observations and Experiments on their respective bleaching Powers," was read by S. Witter, Esq.

The paper commenced with stating the methods of preparing the oxymuriates of lime, potash, and magnesia, for the purposes of commerce: the formation of the dry oxymuriate of lime on the large scale, with analytical experiments on its composition, was particularly detailed. To ascertain the proportions synthetically of the constituent principles of oxymuriate of lime, the oxymuriatic gas was detached from its combination, and received through a saturated solution of common salt: the analysis was calculated from the *weight gained* by hydrate of lime during its conversion into the oxymuriatic salt, compared with its weight previous to that operation. Corroborating the correctness of the analysis, *two* proportions of lime were uniformly found combined with *one* of oxymuriatic gas, in a *solution* of that effective bleaching agent. The application of Sir H. Davy's views of the nature of chlorine and muriatic acid, to Mr. Dalton's analysis of oxymuriate of lime (described in Dr. Thomson's Annals), harmonized the composition of Mr. Dalton's oxymuriate with that stated in Mr. Witter's experiment: and this circumstance was supposed to favour an opinion which the latter had advanced; namely, that the bleaching strength of the oxymuriates was inseparably connected with the presence of an excess of base, and that the real bleaching oxymuriates *in solution* resemble that class called sub-salts. The neutral oxymuriate of potash was found capable of being restored to its original maximum of strength, by the addition of a slight excess of alkali.

All attempts to prepare the oxymuriate of magnesia in a dry form were stated as unsuccessful, and the liquid obtained in the direct way was found to be too expensive, and tedious in preparation, for general use.

The decomposition of muriate and sulphate of magnesia in the mother liquor of salt works, the former by heat, and the latter by charcoal, was suggested as an œconomical process

process for obtaining magnesia, were the preparation of the dry oxymuriate readily practicable.

The author then proposed processes for obtaining liquid oxymuriates of magnesia and of soda. The process for obtaining the former was recommended to the calico-printer, on the grounds of facility of preparation and comparative œconomy: that for obtaining the latter was proposed to the manufacturer's notice, as promising utility in the arts, from the happy coincidence that occurs in the preparation of oxymuriatic gas; namely, the formation of sulphate of soda in large quantities, a substance essential to the proposed process.

Some experiments were then detailed concerning the respective energies of the oxymuriates in discharging vegetable colouring matter; and also on their action upon the texture of linen fabric, the cohesion of which, by using concentrated solutions of each, was found completely destroyed, while no apparent injury was observed to result from the action of the diluted liquid of the bleacher, although the cloth was boiled therein for some hours. Concerning the muriate of lime, it was found that in no degree of strength was it injurious.

The paper concluded with a detailed account of the comparative expense of the oxymuriates enumerated, and with some observations on the objections urged against the oxymuriate of lime.

April 21. The Secretary read a paper entitled "Facts and Experiments relating to Fiorin Grass," by the Right Hon. G. Knox, President of the Society.

The intention of the author had been to present a minute detail of his attempts to analyse that interesting vegetable; but on account of the exaggerated reports that began to circulate concerning his inquiries, it became a matter of necessity to state the facts as far as he had ascertained them *.

The author first adverted to the uncertainty which attends drying the grass: it was observed that at the temperature of 212° or lower, the formation of an empyreumatic oil was evident; while at a still lower temperature it was apparent

* This necessity was further increased by an unfortunate accident which occurred to the author during his experiments, the consequence of which was that he was compelled to relinquish the investigation. The period when he might be enabled to resume it being uncertain, he preferred communicating the present statement, although much more imperfect than what he at first intended; beside that the general analysis became less necessary, as it is reported that such will shortly appear in Sir H. Davy's Agricultural Lectures now in the press.

that

that the whole of the water could not be removed. The author then stated objections which arise when certain other methods are employed, the consideration of which occasioned in him a distrust of his estimate of the vegetable soluble matter.

The general method employed was to dry a certain quantity of the grass, to digest it in water, and dry the residuum in the same temperature as at first: the weight lost in these processes showed the quantity dissolved, and this quantity generally agreed pretty exactly with the solid extract obtained by evaporating the filtered water. In this manner, with but little variation, 25 per cent. of extract, that is, of soluble nutritious matter, was obtained.

It was then stated that the author after many attempts was not enabled to obtain sugar in the insulated form; but that by a complicated process he separated what is most probably saccharine matter of a peculiar nature, amounting to 10 per cent.: this result was corroborated by the action of alcohol on the extract.

But whether the saccharine matter exist in the grass as sugar or as a peculiar substance, it is certain that alcohol is producible from it in no very inconsiderable quantity. It was stated that different persons who conducted the process for the author produced different quantities. On one trial made with more accuracy than the rest, and under the author's immediate inspection, twenty-six ounces and a half of spirit (S. G. 930) were obtained from 27½ avoirdupois pounds of the grass, which did not amount to one half of what was produced when large but proportionate quantities had been employed, and in the hands of experienced distillers.

The formation of 26½ ounces of spirit certainly does not at first sight seem to countenance the quantity of saccharine matter as above stated. Had there been 10 per cent. present, the whole quantity in 27½ pounds must be 19,200 grains: these, according to Thenard's experiments, would produce 9,856 grains of alcohol (S. G. 791), whereas but 4,580 were really obtained. The latter quantity would indicate the existence of no more than 4½ (nearly) per cent. of saccharine matter. But this offers no sufficient objection; it rather offers a presumption in favour of the abovementioned suggestion, namely, that the sugar does not exist in an insulated form, but in such a combination perhaps as to constitute a peculiar proximate principle.

IMPERIAL

IMPERIAL INSTITUTE OF FRANCE FOR THE YEAR 1812, DRAWN UP BY M. CUVIER.

Mineralogy and Geology.

The fossil remains of organised bodies continue to occupy the attention of naturalists.

M. Traullé, of Abbeville, has presented to the Class the petrified head of a small *cetacea*, which seems to have belonged to the whale genus, and which was dug up in the basin of Antwerp. Count Dejean has sent one similar, and from the same place, to the Museum of Natural History. There were also found at the same time a great number of vertebræ of animals of the same class, and several shells.

M. Traullé also presented a portion of the lower jaw of a rhinoceros, found in the sandpits of the valley of the Somme, in the environs of Abbeville.

M. Daudebart de Ferussac, a young military officer who has visited most parts of Europe, has profited by his leisure to notice fossils; and as he has made a particular study of the shells found in fresh water, he applied himself particularly to that sort of soil in the environs of Paris, exposed by Messrs. Brongniart and Cuvier, which, as containing nothing but fresh-water shells, appeared to these naturalists not to owe its origin to the sea, like most other secondary formations.

M. de Ferussac has found similar strata, containing the same shells and composed of the same substances, in the south of France, in several provinces of Spain, in Germany, and even in Silesia; so that it seems to be no longer doubtful that it is formed everywhere.

M. de Ferussac, in order to give more precision to his observations, turned his attention to the shells themselves, determined the species with much rigour, and gave some good observations on the variations which they may undergo, and several correct notions which may distinguish the genera.

M. Cuvier has published in 4 volumes, 4to. with many plates, a collection of all his memoirs on the fossil bones of quadrupeds. He describes 78 species, 49 of which are undoubtedly unknown to naturalists, and of which sixteen or eighteen are still doubtful. The other bones found in recent soils appear to belong to animals which are known. In a preliminary discourse the author details the method which he pursued, and the results which he obtained. Reasoning upon facts which he has discovered, it seems to him that the earth has undergone several great and sudden revolutions, the last of which (not more than 5 or 6 thousand

years

years ago) has destroyed the countries then inhabited by the species now living, and presented as a habitation to the feeble remains of these species, continents which had already been inhabited by other beings, which an anterior revolution had swallowed up, and which reappeared in their present state at the time of this last revolution.

[To be continued.]

XLIX. *Intelligence and Miscellaneous Articles.*

Mr. William Shires, formerly nautical master in the royal navy, assistant to Mr. Sanderson, the mathematical examiner, &c. having reflected upon the little satisfaction usually gained by the common definitions of fluxions and fluents, has taken upon himself to illustrate the same by contemplating the augmentation of the falling body, whose fluent is the square of the time, and whose fluxion is its root = the time; whence, if the augmentation was less than the square of the time, the value of the fluent would be a rectangle; but if the fluent was more augmented than the square of the time, the fluent would be equivalent to a solid; and if uniform, or viz. not augmented at all, the fluxion and fluent would both unite and become one, in which the uniformity or time only would be measured.

Having found these definitions to answer the intended purpose, Mr. Shires intends to publish a small work on this subject.—He considers no other definitions requisite at present.

LECTURES.

Theatre of Anatomy.—Lectures on Anatomy, Physiology, Pathology, and Surgery, by Mr. John Taunton, F.A.S. Member of the Royal College of Surgeons of London, Surgeon to the City and Finsbury Dispensaries, City of London Truss Society, &c.

In this Course of Lectures it is proposed to take a comprehensive view of the structure and œconomy of the living body, and to consider the causes, symptoms, nature, and *treatment of surgical diseases*, with the mode of performing the different surgical operations; forming a complete course of anatomical and physiological instruction for the medical or surgical student, the artist, the professional or private gentleman.

An ample field for professional edification will be afforded by the opportunity which pupils may have of attending

attending the clinical and other practice of both the City and Finsbury Dispensaries.

The Summer Course will commence on Saturday, May the 22d, 1813, at Eight o'clock in the Evening *precisely*, and be continued every Tuesday, Thursday, and Saturday, at the same hour.

Particulars may be had, on applying to Mr. Taunton, Greville Street, Hatton Garden.

LIST OF PATENTS FOR NEW INVENTIONS.

To Frederick Hanck, of High Holborn, in the county of Middlesex, musical instrument-maker, for his improvements in musical instruments.—3d March 1813.

To Joshua Stopford, of Belford, in the county of Northumberland, clerk, for his mangle, intended to be called The complete family accommodation mangle, for mangling linen and other cloths.—3d March.

To William Mitchell, surgeon, late in Ayr, now in Edinburgh, for his important discovery in the manufacture of soap.—3d March.

To Benjamin Merriman Combes, of Fleet Street, in the city of London, ironmonger, for his improved apparatus for the cooking or dressing of victuals, and possessing other advantages in lessening the consumption of fuel.—9th March.

To George Duncan, of Liverpool, in the county palatine of Lancaster, rope-maker, for his several improvements in the different stages of rope-making, and in machinery adapted for such improvements.—13th March.

To Sigismund Rentzsch, of George Street, St. James's Square, in the county of Middlesex, watch-maker, for his hydrostatical or pneumatical chronometer.—13th March.

To Robinson Kitto, of Woolwich, in the county of Kent, gentleman, for his double coned revolving axle for carriages.—13th March.

Meteorological Observations made at Cambridge from March 18 *to April* 8, 1813.

March 18.—Fair day, with lofty and confused *cirrus* scattered about, and flocks and lowering *cumuli* below. Wild geese in large flocks pass over towards the northwest. Wind SW. Therm. 7 A.M. 40°, 2 P.M. 58°, 11 P.M. 47°. Flimsy clouds aloft.

March 19.—Thermometer at noon 61°, at 11 P.M. 40. Clouds and sunshine too; the atmosphere hazy; *cumulostratus, cirrus*, &c. confused aloft. Wind south-westerly.

March

March 20.—Heavy clouds and cool wind; some small rain in the evening was succeeded by a clear cool night. Thermometer 2 P.M. 45°.

March 21.—Fine clear morning. Barometer 29, 90. Thermometer at 1 P.M. 56°. During the day linear and other *cirri* appeared at no great height. I observed about eleven o'clock in the morning an unusual inversion in the order of the clouds, a long *cirrus* moving rapidly in a north wind, at right angles to its length, while at one end of it cirrose fibres pointed to the N. and at the other end to the E. In a higher region flimsy *cumuli* moved in a south wind, and higher came over large flimsy and plumose beds of *cirrocumulus* in NW. wind; afterwards *cirrocumulostratus* formed, and the sky became clouded at intervals: the wind was strong below from NW.

March 22.—Cloudy morning, followed by small rain, after which rainy features of the different modifications appeared. In the evening large confused *cirrocumuli* with some bars of *cirrostratus* appeared. Therm. 5 P.M. 50°. Wind calm. The night became clear, with a breeze; and Thermometer 39° at 11 P.M.

March 23.—Clear morning; afterwards *cumuli* formed as usual, increasing towards midday. The night was starlight, but the stars did not shine bright, and there was a lucid *corona* about Jupiter at times. Thermometer 11 P.M. 36°. Wind westerly.

March 24.—Cloudy morning, followed by gentle rain which continued through the day. Thermometer 11 P.M. 46°.

March 25.—Clouded and some rain, but it held up in the evening.

March 26.—Fair day; *cirrus*, &c. in bands stretched along. *Cirrocumulus* of loose indefinite kind aloft; *cumuli* sail along lower; the *cirri*, &c. seemed to be diminished in proportion as the *cumuli* increased in size and density. Towards evening the *cumuli* disappeared, and high *cirrocumulus* with obliquely descending bands of *cirrus* appeared. Therm. 11 P.M. 36°.

March 27.—Fine day, with crimson forms of *cirrus* in the morning; through the day large and dense *cumuli* formed, and at times obscured the sky. Fine dry warm night. Thermometer at 11 P.M. 48°, but much higher during the day.

March 28.—Clear morning, *cirri* flimsy and changeable with *cumuli* through the day; *cumulostratus* broke out, when the *cumuli* below *cirri* increased in size and density.

Towards

Towards evening the *cirri*, &c. aloft were lost, and *cumulostratus* covered the sky. Therm. at $3\frac{1}{2}$ P.M. 65°. at 11 P.M. 53°. There was a strong electric smell at night like the smell after showers have fallen on dry ground in summer.

March 29.—Much cloud. Therm. 60° at 2 P.M. at 11 P.M. 54°.

March 30.—Clouds and rainy in the morning; it held up in the afternoon. Thermometer 11 P.M. 43°. Wind westerly.

March 31.—Clouded morning; when it cleared, large *cumuli* and *cumulostrati*, and *cirri* of different shapes scattered above, of the rain-bearing kind. Therm. 11 P.M. 44°.

April 1.—Clouded morning, with small rain; fair towards evening; *cirrus*, &c. Therm. 11 P.M. 37°, and starlight.

April 2.—Cloudy, then fair; *cirrus* in a state of fusion above large *cumuli*, &c. Therm. at 11 P.M. 34°.

April 3.—Cold wind and slight showers of snow, which did not lay on the ground; in the intervals it was fine; the *cumuli* were very rocklike and tuberculated in the middle of the day, with *cirrus* above, and scud flying along below them; some of the latter passing rapidly along in the wind in the evening, was in such a state of fusion that it looked like loose and light-coloured smoke. For several days past the wind has been southerly, and to some electric peculiarity is probably owing the low temperature of the air with a wind from that quarter. Clear cold night. Therm. at midnight 34°.

April 4.—Clear, and *cumuli* through the day; at night, some flimsy large features of *cirrocumulus*, &c. and a burry moon. Thermometer 51° at 4 P.M., at 11 38°. Wind southerly.

April 5.—Small rain and warmer.

April 6.—Small rain and warmer still; fine evening, but clouded over. Therm. at night 49°.

April 7.—Fair day, and tolerably warm; various clouds.

April 8.—Fair, with *cumuli* and *cirri*, and very warm. By night a *halo* appeared about the moon, with a nearly clear sky, that is, no visible definite cloud. Thermometer 11 P.M. 52°. Wind S.

Corpus Christi College, Cambridge,
April 8, 1813.

THOMAS FORSTER.

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For April 1813.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
March 27	45	57	49	30·50	47	Fair
28	52	62	53	·47	43	Fair
29	50	59	47	·32	29	Fair
30	52	50	46	.08	20	Rain
31	47	54	42	29·80	36	Fair
April 1	44	44	39	·20	0	Rain
2	40	50	37	·34	28	Stormy
3	34	35	36	·62	29	Hail-storms
4	35	46	37	·84	36	Fair
5	34	50	49	·85	0	Rain
6	50	56	50	·88	29	Cloudy
7	51	55	49	·92	33	Cloudy
8	54	66	52	·98	47	Fair
9	55	67	50	30·03	70	Fair
10	50	63	47	·10	62	Fair
11	46	55	40	·18	52	Fair
12	43	63	54	·12	70	Fair
13	50	64	46	·30	69	Fair
14	47	60	45	·27	60	Fair
15	46	67	49	·10	75	Fair
16	45	68	55	29·98	60	Cloudy
17	50	61	42	·84	52	Fair
18	43	55	54	30·20	47	Fair
19	54	63	55	·12	46	Fair
20	55	64	43	·15	48	Fair
21	44	56	42	·09	40	Fair
22	45	45	40	·11	36	Hail-storms
23	40	47	39	·12	47	Ditto
24	39	46	40	·14	40	Cloudy
25	40	50	45	29·98	0	Rain
26	45	55	40	·97	37	Fair

N.B. The Barometer's height is taken at one o'clock.

L. *On the Equilibrium of a Combination of Beams, Blocks, &c.; and on the Polygon of Forces.* By JOHN SOUTHERN, *Esq. of Soho near Birmingham.*

To Mr. Tilloch.

SIR,—ALTHOUGH several authors, who have written on Arches, Roofs, &c. have treated the subject of Equilibrium very extensively and with great adroitness, I have not seen in any work a problem of the precise nature of that which herein follows; and which I conceive may be of considerable utility in practice, being easy of application.

In all those problems bearing on the present point, that I have noticed, the centre of gravity of each block or beam is supposed to be in the right line that joins the points of abutment; but cases occur in practice in which that does not take place, and to these is the present problem peculiarly adapted, though it is advantageously so to others: for instance, the rafters and slates of roofs have their common centre of gravity higher than the rafters, though it is these which sustain the whole; and fig. 3. herein referred to will point to other cases; and it will be seen that this circumstance is material to their position of equilibrium.

The language of authors on this subject is not always distinct, and is very likely to have been mistaken in regard to the words *bars* and *beams*, which are frequently used in problems of the kind in question under the supposition of their possessing *no weight*, without its being so expressed; and even when it is, it does not appear to me to have been properly treated.

All the authors I have consulted, speaking of a number of beams supporting each other in equilibrio, say the forces are in the directions of the beams; and, certainly, when these have no weight, but are merely props sustaining weights at their angles, this proposition is true; but allow the bars or beams to gravitate, and to be the only gravitating matter, and their forces exist at the angles, which the propositions do not develop: for though some of the authors alluded to show what portion of the weight of each beam lies on the angle, and then direct the consideration to follow as if actually united weights were substituted for them; yet it must be allowed that this is not matter of fact, and that neither the quantum of force, nor its direction, with which each beam presses on its contiguous ones, is shown, as I trust will soon appear.

 Let

Let the beams AB, B′C, and C′D, fig. 10. be connected together by cords BB′ and CC′, and freely suspended from A and D, and suppose them by their gravity to have taken the figure represented, and consequently that of equilibrium. Continue the lines BB′ and CC′ both ways, and their intersection at t will necessarily be in the vertical that passes through the centre of gravity of the beam BC at b. Let a and c be the centres of gravity of the beams AB and C′D, from which let fall the verticals a s and c u, cuttting the lines B′B and C′C′ continued, in s and u. Draw sA and uD. The directions of the forces which sustain the beams AB will be sA and sB; of those which sustain B′C, tB′ and tC; and of those which sustain C′D, uC′ and uD. But obviously and necessarily these are not parallel to the beams, as generally shown in diagrams of this kind: for taking any individual intermediate beam, the directions of those which adjoin it not necessarily intersecting each other in the vertical that passes through its centre of gravity, any forces supposed to act in their directions to sustain it, are not qualified to produce the equilibrium, and therefore do not subsist in fact.

From s continue the line tBs to n, and parallel to uCt, and Du; draw sp' and sr'. The sides An, ns and sA of the triangle Ans are proportional to the weight of the beam AB, to the force acting at B (= the tension of the cord B′B′) and to that acting at A, and in the same directions. The sides np', $p's$, and sn of the triangle $np's$, are proportional to the weight of the beam B′C, to the force acting at C (= the tension of the cord C′C) and to that acting at B′, equal and contrary to that acting at B (= the tension of the cord B′B) and in parallel directions. Also the sides $p'r'$, $r's$, and sp' of the triangle $p'r's$ are proportional to the weight of the beam C′D, to the force acting at D, and to that at C′ equal and contrary to that acting at C (= the tension of the cord C′C′) being in parallel directions.

Now,

Now, if the cords $B\acute{B}$, $C\acute{C}$, were shortened so as to bring the ends of the beams to touch, keeping the latter parallel, they would take the figure $A\,B\,\ddot{C}\,\ddot{D}$, and the directions and value of the forces would remain the same. If this latter figure be inverted, or rather the beams thereby represented, it is known that the forces which have been shown to take place, would be merely changed from those of tension to those of compression, and the short lines drawn across the angles at A, B, $\ddot{C}$ and $\ddot{D}$ at right angles to the forces which connect the beams, are such as, when inverted with the beams, their ends should conform to, to attain any degree of stability; but here is no indication of these lines (representing planes) bisecting the angle formed by the contiguous beams, as one author has informed us they ought.

By these remarks, I do not intend in the smallest degree to impute blame to those authors I have had in view, but merely to point out what appears to me erroneous or defective.

I now come to the principal object of this letter.

Problem.

To put any number of blocks or pieces of timber, stone, &c. whose weights and centres of gravity are given in equilibrio in a vertical plane, in any given order, so that any two of them shall have a given angular position.

Let AB, BC, CD, DE, EF and FG, fig. 1. (Plate IX), be the blocks which are required to be placed in equilibrio in the order just mentioned, in such manner that the spires of CD and EF shall be vertical*; and let right lines be drawn from A to B, from B to C, C to D, &c. the terminations of which are the points of contact or abutment on each other. Also let the points a, b, c, &c. be the centres of gravity of the different blocks. From the extremities of the right line AB draw to the centre of gravity a the right lines A a, B a, and thereby a triangle will be formed of which AB is the base, and a the vertex. Also from the extremities of the line BC draw to the point b, the lines B b, C b forming the triangle B b C, of which the base is BC, and b the vertex; and proceed in like manner with the other blocks, forming the triangles C c D, D d E, E e F, and F f G.

* Which is equivalent to giving the elevations of their base lines, or angular position.

Draw a vertical line ACE, &c. fig. 2. on which set off AB, BC, CD, &c. proportional to the *weights* of the blocks respectively, and in the order in which they are to be placed in equilibrio; on the portions AB, BC, CD, &c. of this line as bases erect triangles similar to the original ones on the blocks, *so* that the vertices of these similar triangles be on the remote or on the proximate side of their bases, in respect of S, as, on the blocks they are above or below the base lines; (S being on the same side of the vertical line AG, as the block AB is on in respect of the other blocks); *and so* that the vertex a, which on the block AB is nearer the point B than A, shall, in the erected triangle, fig. 2. be nearer the point A than B; *so* that the vertex b, which on the block BC is nearer B than C, shall in the erected triangle be nearer C than B, and so on: thus, the erected triangles, fig. 2, will be *reverse* of the original ones, though similar.

Through the vertex c, fig. 2, draw c *c* towards S crossing the vertical AG in *c*, so that the angle C*c*S may be equal to the angle C*c*m of the spire-block CD, fig. 1; *c*, being at the intersection of a line (which is to be vertical) through the middle of the spire m, with the base line CD. Also, through the vertex e, of the erected triangle EeF, fig. 2, draw e*e* towards S, crossing the vertical AG in *e*, so that the angle E*e*S may be equal to the angle E*e*n of the spire-block EF, fig. 1; *e* being at the intersection of a line (which is also to be vertical) drawn through the centre of the spire n, with the base line EF. The intersection of these lines c*c*S and e*e*S, fig. 2, continued, establishes the point S; from which draw lines to the vertices of the other triangles, as Sa, S*b*, S*d*, &c. which will respectively be the angular positions, in respect of the vertical of the base lines of the blocks AB, BC, CD, DE, &c. fig. 1, when placed together in equilibrio, and then they will take the form as shown fig. 3; wherein AB is parallel to Sa, fig. 2; BC is parallel to Sb; CD is parallel to Sc, &c. &c; and the spires m and n are by construction vertical.

From S, fig. 2. draw also lines SA, SB, SC, &c. which will represent the directions and quantities of the forces which sustain the blocks in equilibrio: thus, the block AB, fig. 3, is sustained in its position, by a force acting against its foot at A, proportional to the line SA, fig. 2, and in a direction parallel to the same; by a force acting against the end B of the same block, proportional to the line SB, fig. 2, and in a direction parallel to BS; and by the weight of the block itself, taken proportional

tional to the portion AB of the vertical by construction. *Also*, the block BC, fig. 3, is sustained in its position, by a force acting against B, (being the reaction of the end B of the block AB) proportional to the line SB, fig. 2, and in a parallel direction to SB; by a force acting against the end C proportional to the line SC, fig. 2, and in a direction parallel to CS; and by the weight of the block itself taken in the construction proportional to the portion BC of the vertical. And so for the other blocks. The short lines in fig. 3, drawn across the ends of the blocks at A, B, C, D, &c. show the section of the planes of abutment, and are therefore at right angles to the directions of the forces acting there, or to SA, SB, SC, &c. fig. 2.

Demonstration.

Similar to the triangle AaB, fig. 1, make S*ga*, fig. 2, which is therefore similar to though reverse of BaA, and will represent the block AB in its position of equilibrium in fig. 3. Through *g*, the representative point of the centre of gravity of the block draw a vertical *g o v*, which will be parallel to ABG, and from *o*, where SA is intersected by it, draw *oa*. By mechanics it is known that if a body S*a* be kept in equilibrio by two forces acting at its ends, the directions of these forces must intersect each other in the vertical line that passes through the centre of gravity of the body: hence, if one force act on the body at S in the direction SA crossing the vertical line at *o*, the other force at *a* must act in the direction *a o*; and these forces and the weight of the block will respectively be proportional to the lines *o*A, *a o* and A *a*. The triangles A*a*a and B*a*a, will respectively be similar to, but reverse of those *a v g* and S*v g*: for the angle at A of the first of these triangles, and at B of the second, are, by construction, equal to that at *a* of the third, and to that at S of the fourth triangle; and the angles at *a* of the two former triangles are, because of the parallelism of the verticals *g o v* and AG, respectively equal to those at *v* of the two latter: whence A*a* : AB : : *av* : *a*S : : A*o* : AS, and therefore SB is parallel to *oa*. Wherefore AB, BS and SA are respectively proportional to A *a*, *a o*, and *o* A, and consequently to the forces which keep the block AB, fig. 3, in equilibrio.

The same reasoning will apply to the block	BC CD DE &c.	by substituting for the letters A, B, a, *a*, the letters	B, C, b, *b* C, D, c, *c* D, E, d, *d* &c.

and

and by retaining the letter g for the representative centre of gravity of the similar triangles of which S is at one of the angles in fig. 2; the letter o, for the intersection of the vertical line drawn through g, with the line SB, SC, SD, &c. and v, for the intersection of the same vertical with the line Sb, Sc, Sd, &c.

That is to say, if the blocks are applied to each other in fig. 3, so that the base line of the triangle in each shall have a position parallel to its representative in fig. 2, the mutual forces arising from their gravity and the resistance of the abutments will keep them in equilibrio; because the line SB, which is proportional to one of the forces that keep the block AB in equilibrio, is also the same which is taken to represent the opposite force which helps to sustain the block BC; action and reaction being equal and opposite,—so SC is taken for one of the forces which keep the block BC in equilibrio; as it is also for one of those which keep the block CD in that state,—and so of the rest.

To find the centre of gravity of the frame.

From A, fig. 3, in the direction of the sustaining force there (being parallel to SA, fig. 2) draw Ag'. From C, in the direction of the sustaining forces there (being parallel to SC, fig. 2,) draw Cc', till it intersect the line Af' in c'. In like manner proceed to draw lines from D, E, F and G, in directions of the forces acting at those points (being respectively parallel to SD, SE, SF, and SG, fig. 2) which will intersect the line Ag', at the points d', e', f', and g'. From these intersections let fall vertical lines $c'o$, $d'p$, $e'q$, $f'r$ and $g's$. In the vertical $c'o$ will be found the centre of gravity of the two blocks AB and BC.

For it is known by mechanics, that if a number of bodies be sustained in equilibrio by two forces, their directions must intersect each other in a vertical line that passes through the common centre of gravity of the bodies.

It will also be found in the line ab joining their respective centres of gravity; and the intersection o of these lines will be the common centre of gravity of the two blocks. Also, for similar reasons the centre of gravity of the three blocks A to D will be in the vertical $d'p$; it will also be in the line which joins the common centre of gravity of the two first blocks, and that of the third CD, and at the intersection p of these lines will be the centre of gravity of the three first blocks. In like manner the centre of gravity of

of the four blocks AE	will respectively be at the intersection of the verticals	$e\,q$	with the lines	$p\,e$	at	q
of the five ditto . . AF		$f\,r$		$q\,f$		r
of the whole . . . . AG		$g\,s$		$r\,g$		s

If it be desired to continue this archiform combination by a seventh block AZ, fig. 3, adjoining to the first so as to extend to the horizontal line ZG, draw from A the line AZ, any where between the direction A g of the sustaining force there, and a vertical let fall from A. The length of the block will be thus ascertained. From S, fig. 2, parallel to ZA, fig. 3, conceive the line Sz to be drawn; and z, will therefore be the representative point of the centre of gravity of this additional block; its weight may be of any practical amount greater than Az, so that its centre of gravity shall in the line AZ, fig. 3, be at such a distance from Z in proportion to ZA, as the weight represented in fig. 2 by Az, is to the whole weight of the block. Therefore if it be homogeneous and prismatical, its weight will be double that represented by Az; if it be wedge-form, with its broad end towards A, fig. 3, its weight will be $\frac{3}{2}$ of Az, fig. 2. And from the given length and weight thus found, and its density, its other dimensions may be readily calculated *.

It will appear, on consideration of this problem, that if the spire block, CD had had its base line at right angles to the spire, and had been uniform in its figure, that is, if the spire had arisen from the middle of and perpendicular to its base, its representative in fig. 2, Sc would have been at right angles to the vertical AG; and blocks similar to DE, EF, and FG, but reverse, being placed in lieu of AB and BC, would not only have made the whole figure uniform, having a middle and two side spires, but (attending to the directions of the problem in ascertaining the positions) the whole would also have been in equilibrio. Further, if the centre-block of this uniform combination be now made twice as heavy, it is plain that it will require, in order to be supported in equilibrio, the side blocks to be also twice as heavy—or, which will be equally efficacious, that

* I have made a model of wood much like fig. 3, whose span ZG is about 8 feet, the extreme blocks are wedges, with their edges downward, on which the whole stands. The planes at the joinings are about 1½ inch deep and 3 to 4 inches in the horizontal direction (at right angles to the plane of the figure). When a moderate pressure is applied to any of the blocks, and then suddenly withdrawn, the whole vibrates on the edges of the two extreme blocks.

 another

another double set of side blocks similar and equal be applied to the central one; and if these latter be applied at right angles to the former, there will be a combination of blocks, having a central spire and four surrounding ones on the haunches of the side blocks, resembling in a considerable degree (if I forget not) the stucture at the top of the old church at Newcastle on Tyne; and showing how such a construction may be effected without the aid of concealed iron work, provided the horizontal thrust at the feet be resisted.

The following problem being naturally connected with this subject, I take the liberty here to add.

When three forces conspire in their action on a point, which is thereby kept at rest, the proportional quantity of each is ascertained by the well known "*triangle of forces*," whose sides are parallel to the directions of the forces; the present problem is to ascertain, when any greater number of forces act on a point (in the same plane) whose quantities and directions are given, what their united effect upon that point is—or, what other force acting in that point will counteract the given forces so as to keep the point at rest. This is performed by a figure which I think may be aptly called the *polygon of forces*.

Let SA, SB, SC, and SD, fig. 4, be the directions, and their lengths the quantities of four forces acting on the point S; it is required to know what their united effect is on the point S; or, what other force acting on that point, and in what direction, will counteract them, and keep it at rest.

From the outer end of any of the lines as SA draw parallel to the direction of the adjoining force SB the line A b, equal in length; from b, parallel in direction to the next force SC, and equal in length draw b c; from c in like manner draw c d parallel and equal in length to SD; complete the polygon by drawing the line d S; which is the direction, and its length the measure of the force that will counteract the four given forces; and it is therefore the measure of the effect of those forces, and the direction of that effect is S d. Therefore if the line d S be continued to E making SE=dS, it will represent the required counteracting force in relation to the given point S, in the same manner as the *given* forces are represented. Thus the forces SA, SB, SC, SD and SE acting on the point S with energies proportional to and in the directions of those lines will keep it at rest, or be in equilibrio.

If

If instead of taking the forces in angular succession, any other order had been observed, the resulting force would have been precisely the same: as if the order had been SA, SC, SD and SB, the figure would have been S A c d d, wherein A c is parallel and equal to SC; c d, parallel and equel to SD; and d d parallel and equal to SB; showing the equivalen. force S d the same as before.

Demonstration. Draw the lines S b and S c; by the composition of forces S b is equivalent to the forces SA and SB. Also S c is equivalent to S b and SC, and therefore also to the three forces SA, SB and SC; also S d is equivalent to S c and SD, and therefore equivalent also to the four forces SA, SB, SC, and SD.

A number of useful corollaries might hence be drawn; but having already exceeded the limits I first proposed, I subscribe myself,

Sir, your most obedient servant,

Soho, near Birmingham, March 1, 1813. JOHN SOUTHERN.

LI. *On Egyptian Ophthalmia.* By WILLIAM ADAMS, *Esq. Surgeon and Oculist.*

To Mr. Tilloch.

SIR,—THE violence and extensive dissemination of the purulent inflammation of the conjunctiva, under the appellation of Egyptian ophthalmia, has made it too generally known to require now any description of it; and though it has certainly become less destructive since very copious bleedings have been employed, yet it has seldom, if ever, been completely subdued at its commencement by this or any other practice. Under this state of practical knowledge, in the treatment of a disease, the rapidity of whose progress professional men have such frequent occasion to witness, it became a desideratum to obtain some remedial process which should arrest the morbid actions before the eye had sustained any serious injury. The success which has attended a mode of treatment suggested by me for several persons labouring under this disease in the St. Pancras workhouse, has made me hope that this desideratum is now obtained.

The Egyptian ophthalmia had existed, in a most active and virulent state, among the children in the above establishment, for nearly two years; and had infected the attending surgeon and the nurses. During this period, the modes of treatment recommended in the publication of an

eminent

eminent oculist, were tried to their fullest extent, under the superintendance of himself and his son; and, though the violence of the complaint was reduced, it was not eradicated.

On my being officially requested, by the acting committee of this parochial establishment, to undertake the treatment of these patients, I stated to the attending surgeons of the house, Messrs. Uppom and Lewis, of Warren-street, Fitzroy-square, that some facts had come within my knowledge, which led me to believe that this alarming disease might be stopped in its progress by the energetic use of emetics. Mr. Lewis, under whose care the ophthalmic patients princially came, undertook to superintend the experiment in the first cases of the acute form of the disease which should be brought to the infirmary.

The process was simply to give such a quantity of emetic tartar as would keep up *constant sickness and vomiting* for eight or ten hours; at the same time applying within the eye-lids some of the ung. hydrarg. nitri oxyd. This succeeded perfectly. Vomiting was then tried without the ointment, and was equally successful.

The following extract from a document, written by Mr. Lewis, and sent to His Royal Highness the Commander in Chief, states a series of facts explanatory of the process, and its success.

"During the first fortnight of the present month (January 1813), thirteen patients with the Egyptian ophthalmia were admitted into the infirmary of the St. Pancras workhouse. The treatment suggested by Mr. Adams was immediately put in practice, and perfectly succeeded in removing the disease in a few hours in every case except one, that of John Kenny. This man had the ophthalmia three months since. By large bleedings and blisters, his eyes were preserved, and the acute inflammation subsided; but the disease of the inner membrane of the eye-lid still remained, and every trifling cold caused a relapse of violent inflammation. At this time he had an attack of the acute disease in one eye. In less than eight hours after the method proposed by Mr. Adams had been employed, the inflammation was completely removed. A few days after, the other eye became similarly affected. For five days he perversely delayed the methods which had preserved his other eye; when extensive ulceration of the transparent cornea took place, and vision in this eye was entirely destroyed.

"In all these cases, the extreme pain which attended the onset of the disease, together with the rapidly increasing inflammation,

inflammation, became almost immediately arrested, by Mr. Adams's mode of treatment, and the patients have been generally discharged from the infirmary in two or three days, with their eyes in as healthy a state as they were before the attack of the disease."

Since the above was written, Mr. Lewis has employed this practice, with similar success, both in private practice and in the infirmary.

The powerful results of this practice, I think, fully entitle it to general consideration; and I hasten to lay them before the public with the view of inducing its extensive adoption, and of obtaining, through the medium of your Journal, the information of that success which I anticipate.

The only direction necessary is to exhibit the antim. tart. in doses adapted to the age and constitution of the patient, *as soon after the commencement of the disease as possible.* The ulcerative process frequently begins in ten or twelve hours after the accession of inflammation; and it is evident that the remedy, when that stage of the disease has begun, must be ineffectual, as is strongly exemplified in the case of John Kenny*.

WILLIAM ADAMS,
Late Surgeon of the West of England Infirmary for curing Diseases of the Eye, instituted at Exeter.

28, Albemarle-street, March 16, 1813.

LII. *On the geographical Position of Lynn, in the County of Norfolk.* *By* EZ. WALKER, *Esq.*

To Mr. Tilloch.

SIR,—THE observations from which the following results were deduced, were made in a room which stands about 206 feet south of the ridge of St. Nicholas's chapel in Lynn, and about 79 feet east of the meridian of the weather-cock upon the steeple of the same building: these distances were measured from the object glass of the transit telescope.

The mean of 27 observations on the meridian altitude of the sun, taken with a sextant of 12 inches radius, made by Mr. Stancliffe, gave the latitude 52° 45′ 23″,76 N. And the

* The importance of this communication, containing a practical fact of such decisive utility, we trust will make a due impression on our readers; and that every opportunity will be taken to ascertain how far its efficacy extends. In the instance before us, which we hope the experience of other practitioners will fully confirm, we see a disease of the most alarming nature, rapid in its progress, and speedily destructive in its results, arrested at its onset, and all its subsequent evils prevented!—EDITOR.

the mean of near 200 observations on the Sun and fixed stars, taken with a mural circle of 15 inches diameter, made by Mr. Troughton, gave the latitude 52° 45′ 25″ N. Hence we may infer that the latitude of the place of observation is 52° 45′ 24″,4 N.

The longitude was found by three different methods: first, by chronometers; secondly, by the eclipses of the satellites of Jupiter; and thirdly, by the transit of Mercury over the Sun, November 9, 1802. The eclipses were observed by a three feet reflector magnifying about 100 times, made by Mr. John Watson; and the transit of Mercury by a 3½ feet refractor magnifying about 80 times.

The longitude of my station found by chronometers, made by Mr. Barraud, is as follows;

No. of Chronometer.		Longitude in Time E. of Greenwich.
		′ ″
183	gave	1 33,9
315	ditto	1 36,3
315 May 23, 1804	ditto	1 35,8
305 July 12, 1804	ditto	1 32,6
312 May 20, 1805	ditto	1 35,1
312 October 3, 1806	ditto	1 36,2
312 Ditto 18, 1806	ditto	1 35,9
Longitude deduced from nine observations on the eclipses of the satellites of Jupiter		1 35,8
Longitude deduced from the transit of ☿		1 35,6
The mean longitude		1 35,2

The chronometers were compared at Somerset House in the Strand, London, by the late Mr. G. Gilpin, and by myself at Lynn. The time here was computed from transits of the Sun and fixed stars over the meridian, observed with a 3½ feet transit telescope, made by the late Mr. Sisson, and greatly improved by Mr. Troughton. The clock, which stands in the same room, was made by the late Mr. James Bullock *.

The observations on the eclipses of the satellites of Jupiter, and those on the transit of Mercury, were compared with corresponding observations made at the Royal Observatory.

Although the longitude derived from nine observations on the immersions and emersions of the satellites of Jupiter, differs but little from the longitude found by the other two methods,

* For an account of the going of this clock, see Phil. Mag. vol. xxxiii. p. 30, and vol. xxxiv. p. 3.

methods, yet these observations differ considerably from one another. In one instance this difference amounts to 42 seconds; and it may be expected that such differences will frequently happen, for the air may be more favourable for observation at one place, than it may be at the other.

But the greatest difference in the results derived from the chronometers is only 03″,6; and if this difference were thrown out of the computation, the remaining five observations would give the longitude as before, within *two-tenths* of a second.

In August 1785, Mr. William Wales, of Christ's Hospital, and Mr. George Gilpin, of Somerset House in the Strand, paid a visit to their friends in Norfolk, and, as they passed through Lynn, called on me to know the time here. On their return to London, Mr Wales favoured me with the result of the astronomical observations which he had made in Norfolk. As he has settled the latitude and longitude of a point of land, interesting to the geography of this county, I look upon this part of his communication of too much value to be lost. I have, therefore, extracted the following article from his letter.

"Christ's Hospital, London, Sept. 2, 1805.

"I found that no part of the county of Norfolk lies to the northward of the latitude of 53°. I could not determine whether the bluff point called Scolt Head, or Holm Point, be the most northerly point of Norfolk. Indeed, I find it is a disputed point amongst those who have had many opportunities of trying.

"A Mr Hendry, of Brancaster, an old coaster, and who has set these points often when one came open of the other, says that Scolt Head forms the most northerly point at high, and Holm Point at low water, as the tide ebbs out further at Holm Point than it does at Scolt. Now, I had six tolerably good observations for the latitude of Scolt Head; and pronounce it to lie in latitude 52° 59′ 31½″ N. and longitude by my watches 0° 44′ 11″ E. of Greenwich.

"I am, sir, with great esteem, &c.

"WM. WALES."

Mr. Wales had two chronometers with him on his Norfolk tour, which he compared at Somerset House, both before he set out and after his return. The longitude of Lynn, by one of his chronometers, was 1′ 35″,1 in time E. of Greenwich; by the other 1′ 40″,4. In this determination he supposed Somerset House, in the Strand, London, to lie 17″ west of the Royal Observatory. The chro-

chronometer which gave the longitude of Lynn 1′ 40″,4 must have gone incorrectly; indeed, Mr. Wales told me that one of his watches had been within the influence of a very strong magnet, in consequence of which its balance was magnetical, and no great confidence could be placed in its performance.

Lynn, April 21, 1813. EZ. WALKER.

LIII. *An Attempt to determine the definite and simple Proportions, in which the constituent Parts of unorganic Substances are united with each other.* By JACOB BERZELIUS, *Professor of Medicine and Pharmacy, and M.R.A. Stockholm.*

[Continued from page 284.]

XII. IRON AND OXYGEN.

THE examination of the degrees of oxidation of iron is in more than one respect of great importance; its determination being particularly concerned in the analysis of almost every mineral. Bucholz has shown by a series of very interesting experiments, that our knowledge of this subject is very deficient, and he has endeavoured to supply the deficiency. But he employed for his experiments common bar iron, which contains a considerable portion of charcoal; and not having taken this circumstance into the account, his results are become erroneous.

A. *Oxide of Iron.*

1.) I dissolved four grammes of harpsichord wire, No. 6, in muriatic acid, applying a gentle heat, and collected the gas evolved by means of rain water. It amounted, together with the atmospherical air of the vessel, to 66 decimal cubic inches, or thousandths of a cubic foot. The gas was burnt, by means of an apparatus arranged for the purpose, in oxygen, which had stood for several days over lime water, so as to be freed from carbonic acid. During this combustion of the hydrogen some carbonic acid was formed; and this acid, when received in lime water, threw down a precipitate of carbonate of lime, which, when placed on a filter, and dried in a heat a little above the boiling point, weighed ·165 gr. Now, according to my analysis, the carbonate of lime contains 43·6 per cent. of carbonic acid; consequently the ·165 gr. contained ·07195 of carbonic acid, the carbon of which weighed ·02 gr. that is, $\frac{1}{2}$ per cent. of the weight of the iron. The solution in the muriatic acid was greenish, and

and not perfectly transparent, but deposited no precipitate after standing a few hours. It was mixed with nitric acid and boiled, in order completely to oxidate the protoxide: the caustic ammonia then afforded a precipitate, which when washed, dried and ignited, weighed 5·74 gr. Consequently 4 gr. of this iron had gained 1·74, 100 parts having taken up 43·5 of oxygen. There was no reason to attribute any part of this addition to the solvent, or to the precipitant, both being volatile; and if they had not been perfectly washed away, they must have volatilised also some of the oxide. Bucholz obtained from 100 parts of iron only 142 of the red oxide; so that either some circumstance in his mode of conducting the experiment must have occasioned a loss, or his iron must have contained much more carbon than mine.

If now we subtract the carbon contained in the iron, there will remain 3·98 gr. of the pure metal, which afforded 5·74 of red oxide; and 5·74 : 3·98 = 100 : 69·34, so that the oxide of iron consists of

Iron . . .	69·34	100·00
Oxygen	30·66	44·25

This subject was so important as to require that the experiment should be repeated. In order to obtain iron of a uniform quality, I had a large nail filed clean and divided into several pieces.

2.) A piece of this nail, which weighed 7·1 grammes, was dissolved in diluted sulphuric acid, and afforded 117 decimal cubic inches of gas, which, when burnt in oxygen, produced ·285 gr. of carbonate of lime, containing ·0344 of carbon, or not quite $\frac{1}{2}$ per cent. of the quantity of iron. In the solution a black powder was deposited, which after drying weighed ·006 gr. and was found to be silica blackened by a little carbon.

3.) Of the same nail five gr. were dissolved in pure nitric acid in a glass flask, evaporated to dryness, and ignited in the flask. The oxide obtained weighed 8·0025 gr. Consequently 100 parts of the iron had taken up 43·25 parts of oxygen.

4.) Another portion, weighing 3·5 gr. was dissolved in aqua regia, and precipitated by caustic ammonia. The precipitate, after ignition, weighed 5·05 gr.; so that 100 parts of iron had taken up 43·75 of oxygen.

5.) I dissolved 5·6 gr. of a thick polished iron wire in muriatic acid. The gas obtained amounted to 93 decimal cubic inches, and afforded, after it had been burned in oxygen, ·225 gr. of carbonate of lime, containing ·0279 gr. of carbon,

carbon, that is $\frac{1}{2}$ per cent., or accurately ·497 per cent. of the quantity of the iron. The solution gave after filtration ·005 gr. of light gray silica. Five grammes of the same wire dissolved in nitric acid, in a glass flask, then dried and ignited in the flask, afforded 7·19 gr. of red oxide of iron, or 43·8 of oxygen to 100 of iron.

6.) Three grammes of the same wire were dissolved in nitric acid, and precipitated by caustic ammonia. The precipitate, after ignition, weighed 4·305; so that it contained 43·5 of oxygen to 100 of iron.

The greatest of these results is that of No. 5, which gives 44·5 of oxygen to 100 parts of iron free from carbon. All the rest vary but little from 44·25, so that the whole uncertainty lies between $44\frac{1}{4}$ and $44\frac{1}{2}$. I have assumed throughout this essay $44\frac{1}{4}$ as the true proportion of oxygen in the red oxide with 100 of iron.

It is a remarkable inference from this experiment, that our common and most malleable iron still contains carbon, which however does not exceed the proportion of $\frac{1}{2}$ per cent. In ill-manufactured bar iron, the quantity is certainly much greater, and hence may arise imperfections, where no foreign substance can be discovered by the usual modes of analysis. The presence of silica, or rather of its base, in malleable iron, may also serve as a proof, that if an ore of iron contains an oxide of any other metal, this metal, which must always be less combustible than silicium, must also continue mixed with the cast iron, and even not be completely separated in the manufacture of bar iron, but always remain mixed with it in a small quantity. And the same must be true of sulphur and phosphorus. Hence it appears how necessary it is, in the examination of the faults of iron, to have at hand the ores and the fluxes employed in its preparation; since substances present in such minute quantities cannot possibly be discovered, unless our attention has been particularly directed to them.

B. *Protoxide of Iron.*

It is generally believed that iron has only two degrees of oxidation, that of the black and that of the red oxide; and that the black, the martial æthiops, or finery cinder, is that into which iron is changed during its solution in acids. Several circumstances however appear to contradict this opinion.

If, for example, we add caustic ammonia to a recently prepared solution of iron in muriatic or sulphuric acid, and prevent the access of atmospheric air, even a considerable excess

excess of ammonia will not in this case be able to throw down the whole quantity of iron; the precipitate is white, and the liquid standing over it retains its former colour. If we admit the air, the fluid is immediately covered with a blue pellicle, which becomes continually thicker, and acquires first a green and then a yellow colour; and the same change of colour takes place in the white protoxide which has already been precipitated. The easiest mode of explaining these appearances would be to suppose that iron has three degrees of oxidation, that the white oxide is in the lowest degree that it is present in these solutions, and that it has in some respects a stronger affinity for the acid than ammonia; but that when the air is admitted, it is further oxigenized, is converted into the black or blue oxide, and precipitates. This explanation I have long thought satisfactory.

If we leave a saturated and recently prepared solution of iron in the muriatic acid, standing undisturbed for some time in a high cylindrical vessel, and exposed to the open air, and then introduce, by means of a glass tube, some drops of caustic ammonia at different heights in the glass, we shall find the precipitate at the top green, below this blue, still lower grayish blue, dirty white, and lowest of all quite white; according to the degree in which the oxygen of the air has been absorbed. If we digest iron filings with a solution of sal ammoniac in a glass nearly full and well corked, a part of the iron will be dissolved in the sal ammoniac, the fluid will become alkaline, and will deposit blue, green, and yellow oxide, when exposed to the air. But with finery cinder the solution of sal ammoniac undergoes not the slightest change.

It was probably on this foundation that Thenard considered the white precipitate produced in these experiments as a protoxide in its lowest state of oxygenization; and as he thought that he found for each oxide salts of different degrees of saturation, he has hence derived an astonishing number of sulphuric and prussic salts of iron.

Bucholz, by a series of laborious and ingenious experiments, has determined the quantity of oxygen of the protoxide of iron to be 23 per cent., 100 parts of iron combining with 29·88 of oxygen. If we calculate from the sulphur contained in the sulphuret at a minimum, the quantity of oxygen taken up by 100 parts of iron should be 29·4 or 29·5. We have further seen that, in the sulphate of the protoxide, 100 parts of acid are united to so much of the protoxide as will furnish 99·22 of the red oxide,

oxide, containing 68·78 of metallic iron. Now, 100 parts of sulphuric acid require, in the base by which they are saturated, 20·29 of oxygen, and 68·78 : 20·29 = 100 : 29·5; a result which agrees very accurately with Bucholz's experiments.

I exposed ten grammes of crystallized sulphate of the protoxide of iron, in a retort, to a heat which was kept below ignition, in order to drive off the water of crystallization: they lost 4·63 gr. They were then kept in a state of ignition, until the acid was completely expelled, and they afforded 2·82 gr. of red oxide; which had consequently been combined with 2·842 gr. of sulphuric acid, and which contain 1·95 gr. of iron. The acid, the iron and the water amount together to 9·422 gr. The remaining ·578 must have been the oxygen of the protoxide; and 1·95 ·578 = 100 : 29·6; so that 100 parts of iron were united to 29·6 of oxygen.

Since in the dry sulphate of the protoxide of iron 100 parts of sulphuric acid are combined with 68·78 of iron, and the oxygen necessary for its oxidation, this oxygen must either be 29·5, according to the foregoing calculation, or 22·125, half of that which is contained in the red oxide, for 100 parts of iron. It is easy to show that the latter supposition leads to consequences which do not agree with the experiments; and the former being supported by two different modes of calculation, we may consider it as sufficiently established that the *protoxide of iron* consists of

Iron	77·22	100·0
Oxygen .	22·78	29·5

The truth of this assertion may be thus demonstrated. If we burn the sulphate of the protoxide in a properly regulated temperature, the protoxide becomes converted into an oxide at the expense of the acid, a part of which is decomposed, while another part passes over undecomposed, or remains behind, according to the degree of heat employed: the salt containing, for 100 parts of sulphuric acid, 84 or 89·07 of the protoxide, and requiring for its complete oxidation in the first case 15·22 parts of oxygen, in the second 10·15. Since in this process the sulphuric acid is only reduced to the sulphurous, 15·22 parts of oxygen would imply that three-fourths of the sulphuric acid, or $76\frac{1}{2}$ parts, but 10·15, that half only was decomposed, and in the latter case we should be able to obtain from the salt oxidated by ignition more than one-fourth of the sulphuric acid unaltered. In order to determine between these alternatives, I took a portion of sulphate of the protoxide, crystallized

stallized in fine grains, having been lately prepared by the solution of iron filings, and well washed from the adherent liquid; I filled with it a small glass retort $1\frac{1}{2}$ inch long and $\frac{3}{4}$ of an inch in diameter, and exposed it in a sand bath to a heat which was gradually raised to ignition. The water of crystallization was collected in a receiver luted to the retort, and the gas was carried off by a tube which opened under water. When the mass had been weakly ignited for about an hour and a half, the evolution of sulphurous acid was almost entirely discontinued, and drops of sulphuric acid appeared in the neck of the retort. I now stopped the process, and suffered the apparatus to cool. The residuum left in the retort was dissolved in water, and afforded a reddish yellow solution, in which caustic ammonia occasioned a red precipitate, without any trace of a mixture of blue or green oxide. The part which remained undissolved in water was dissolved by boiling in pure muriatic acid, and caustic ammonia was added to both solutions. The red oxide thus obtained, weighed, after ignition, 2·4 gr. and was not in the least magnetic, even when it was rubbed into a fine powder, so that it contained no protoxide. This oxide supposes the presence of 2·415 gr. of sulphuric acid in the sulphate. To the solutions in water and in muriatic acid as much more of this acid was added as was necessary for saturating the excess of ammonia which had been employed; and then by means of the salt of baryta a precipitate of 2·92 gr. of sulphate of baryta was obtained, containing one gr. of sulphuric acid; that is, somewhat more than one-third, but not quite half of the whole sulphuric acid, that which was distilled over, as well as that which was converted into oxygen and sulphuric acid gas, being excluded from the account. Since therefore less than three-fourths of the whole quantity of acid is required for the conversion of the protoxide into the oxide at its expense, the protoxide must contain more than 22·125 of oxygen for 100 parts of iron, and the former, not the latter, of the proportions mentioned must be the true one.

Iron therefore has in this respect the same relation to oxygen that sulphur has: in its highest degree of oxygenization it combines with only half as much more oxygen as in its lowest: unless future experiments should show that some lower stage exists in particular combinations of iron, which are yet little known; for example, in the colouring substance of the blood. This circumstance

 stance

stance also explains why none of the salts of the oxide of iron observes the same proportions with any of the known combinations of metallic iron and sulphur.

What may be the difference between the white, the black, the dark blue, and the green precipitates from the salts of the protoxide, I must confess myself unable to explain. Is it not possible that they may be subsalts, the white of the protoxide, the dark blue and green, triple combinations, like the triple neutral salt, and containing the oxide and protoxide in different proportions? This at least appears to me to be the most probable conjecture.

[In a future number, the reader will find a more satisfactory account of the nature of these substances by Mr. Hausmann: *Gilbert*. We have not yet seen Mr. Hausmann's Essay; but in the 6th number of Gilbert for 1811, we find the following remarks of our author: "I am impatient to read Mr. Hausmann's Essay on the Hydrates of Iron: he has communicated to me some results, which however do not agree well with my principles. I have often tried in vain to obtain a pure hydrate of iron. It is therefore impossible to determine the question satisfactorily, since the presence of a third body often influences the quantity of water taken up, as my experiments demonstrate. My opinion of the nature of these precipitates is confirmed by the frequent occurrence of such compounds in nature. The black magnetic iron ores, for instance, contain an oxide of which the oxygen is two, three or four times as much as the oxygen of the protoxide, notwithstanding the powdered mineral exhibits no traces of a red or unmagnetic oxide. The grass-green vitriol of iron, and Prussian blue, are both triple compounds of the protoxide and the oxide with the same acid."]

* * *

From the experiments which I have here circumstantially related, the simple law of chemical affinity, which I stated in the beginning of this essay, is demonstrated in a pretty satisfactory manner. They show also the truth of the relation which follows from this law; that every acid requires an equal quantity of oxygen in every base with which it forms a neutral salt; so that the quantity of the base, by which an acid is neutralised, depends on the capacity of its inflammable radical for oxygen. I consider myself therefore as now authorised to employ these laws in the analysis of the alkalis.

XIII. AD-

XIII. Additions to the foregoing Sections,

Relating to the Sulphuret and the Oxides of Lead, and to the Sulphuric Acid. Extracted by Gilbert *from a Manuscript Communication of the Author.*

In order to obtain lead perfectly pure, I repeatedly dissolved and crystallized the nitrate of the protoxide, until the mother liquor, when slowly evaporated, remained perfectly white, and, when digested with carbonate of ammonia, exhibited no copper upon passing sulphuretted hydrogen through the fluid. Frequently after three crystallizations decided marks of copper were observed. The purified salt, mixed with charcoal dust, was burnt in a Hessian crucible, and the lead obtained, in order to free it from the carbon adhering to it, was kept for some time ignited. When dissolved in the nitric acid, it exhibited no marks of the presence of any foreign substance.

A. *Sulphuret of Lead.*

Twenty-five grammes of this lead, in small pieces, were put into a glass flask with a narrow mouth, with ten gr. of sulphur, which had been kept for half an hour in fusion over a spirit-lamp, and which was introduced while still fluid; they were heated slowly until the glass began to melt, the opening of the flask being closed, when no more sulphureous fumes appeared, with a stopple of charcoal. The sulphuret, which had assumed a crystalline form and a metallic splendour, weighed 28·855 gr.; so that 100 parts of lead had taken up only 15·42 of sulphur. I therefore imagined that all the lead had not combined with the sulphur; and taking ten grammes of the compound, I mixed them very carefully with pure and dry sulphur, and heated them together in a smaller flask, until the glass was softened: but they neither acquired nor lost any weight.

The experiment was repeated with 15 gr. of lead, whence I obtained 17·3125 of sulphuret. Upon another repetition it afforded 17·31 gr. of the sulphuret. Consequently according to these experiments, which agree perfectly with each other, the sulphuret of lead contains

Sulphur	13·36	15·42	100·0
Lead . .	86·44	100·00	648·5

Notwithstanding this agreement, it is still very possible that the quantity of sulphur is here represented as too small, since in so strong a heat it was impossible to keep the vessels air-tight. In my earlier experiments, I found that 100 parts of lead took up from 15·55 to 15·56 of sulphur. The

sulphuret was powdery, and dark gray, without metallic lustre. I at first attributed this to impurities in the lead which I then employed; but I afterwards found that it depended on the presence of hydrogen. I had mixed ten grammes of recently prepared sulphuret of lead with 40 gr. of ignited oxide of tin, and heated the mixture in a small glass retort; hence I obtained, with some sulphurous acid gas, a few drops of water. The same happened a second time; and as I had the instant before ignited both the substances, and had mixed them while still hot, the water could be derived from no other source than from hydrogen in the sulphuret. In fact, when I ignited some sulphuret of lead which had a metallic lustre, and had been prepared in a white heat, together with some oxide of tin, I obtained only a very slight trace of water, which clouded the neck of the retort.

I was hence induced to make some experiments on the *hydrogen* contained in sulphur. In these experiments it was observed, that most powdered substances, which I had freed from moisture by ignition, when they were again exposed to the open air, and then, without any alteration of the moisture or temperature of the atmosphere, after some hours again ignited in a small glass retort, afforded some water in the neck of the retort, which however was only extricated after the temperature had been raised far above the boiling point. Hence it is very difficult in such experiments to avoid the moisture, which adheres mechanically to the substance, and which always makes the result somewhat too great.

Five grammes of sulphur, which had before been dried by fusion over a spirit-lamp, were mixed with fifty of the ignited protoxide of lead, and exposed, in a small glass retort, to a temperature which was gradually raised. The retort was furnished with a small receiver, out of which the sulphurous acid gas was conducted into a glass tube, filled with muriate of lime. When the retort had been ignited for half an hour, it had lost ·9 gr. in weight, while the receiver had gained ·157. The water in the receiver was tasteless, and had a slight sulphureous smell. Hence the sulphur in this experiment had afforded 3·15 per cent. of water. It could not have contained the whole of this quantity as adhering moisture, having been previously in fusion: and the water must have been formed from the hydrogen in the sulphur and the oxygen of the protoxide of lead. There remained in the retort a mixture of sulphate of the protoxide and sulphuret of lead. The quantity of water obtained indicates somewhat less than ·4 per cent.

cent. of hydrogen in the sulphur. Since this quantity of hydrogen bears no proportion to that which is contained in sulphuretted hydrogen, it can only be considered as an accidental impurity, arising from the mode of preparation of the sulphur, and inseparable either by fusion or by sublimation. It appears therefore to be unnecessary to consider sulphur, according to the ingenious hypothesis of Davy, as a triple combination of sulphur, hydrogen, and oxygen; for, if this idea were true, the quantity of hydrogen contained in sulphur would be required to be expressed by a number obtained by dividing the hydrogen of sulphuretted hydrogen by 2, 4, or at most 8; which however is not the case.

B. *Protoxide of Lead.*

Since the composition of the protoxide of lead has served as the foundation of most of my computations, I have endeavoured to examine this body with the greatest possible accuracy in the continuation of my experiments; I have not however been able to remove every difficulty, even after the repetition of the experiments which are now to be described.

I dissolved 25 grammes of the same purified lead, which I had employed in the experiments above described, in a glass flask, in pure nitric acid; I dried the solution in the flask, and ignited the salt carefully, until the air which I drew out of the ignited flask with my mouth through a long glass tube, no longer contained any nitrous vapour. The flask had now acquired an addition of 26·925 gr. in weight: consequently the experiment confirms the first of the former series relating to the composition of the protoxide of lead, and shows that this *protoxide* consists of

Lead . .	92·85	100·0	1298·7
Oxygen	7·15	7·7	100·0

From other calculations, I think I may venture to assert, that if these numbers represent the quantity of oxygen as somewhat greater than the truth, it cannot still be less than 7·633 for 100 of lead. It is unfortunate that the bodies which are the best adapted to afford foundations for computation, take up the smallest quantity of oxygen, so that any unavoidable inaccuracy in the experiments made with them is proportionally of the greater consequence.

I have therefore possibly been in an error when I have set down the oxygen in the protoxide of lead as very precisely equal to half the quantity of sulphur which saturates the same lead.

 C. *Sul-*

C. *Sulphate of the Protoxide of Lead.*

1.) I dissolved 30 gr. of pure lead in nitric acid, put the solution into a platina crucible, with sulphuric acid in excess, dried it carefully, and ignited it. The sulphate of lead weighed 43·9 gr. Consequently 100 parts of sulphuric acid were saturated by 278·77 of the oxide, precisely as in the former experiments.

2.) Thirty grammes of pure protoxide of lead were dissolved in nitric acid, decomposed by an excess of sulphuric acid, and dried and ignited in the platina crucible. The dried protoxide weighed 40·77 gr.; so that 100 parts of sulphuric acid had united with 278·55 of the protoxide. If I omitted to employ the sulphuric acid in excess, a part of it was driven away at a high temperature, by the nitric acid, and I obtained a mixture of the sulphate with the simple protoxide.

3.) I dissolved 15 gr. of the protoxide of lead in nitric acid, evaporated the solution to dryness, dissolved the salt in water, and precipitated the protoxide by the addition of the sulphate of ammonia. A little more sulphate of lead was separated from the clear fluid by the addition of some caustic ammonia. The precipitate, collected and ignited, weighed 20·36 gr.

4.) The same quantity of the same protoxide treated with nitric and sulphuric acid in a platina crucible, gave 20·365 gr. of sulphate of lead.

According to these last experiments, 100 parts of sulphuric acid would saturate 279·59 of protoxide of lead. In my future calculations, I shall take for the proportions of sulphate of the protoxide of lead

Sulphuric acid ..	26·385	100	35·8
Protoxide of lead	73·615	279	100·0

From all this it may be observed, how extremely difficult it is to obtain a perfect coincidence in the results of analytical experiments; a portion of the weight which is scarcely if at all sensible on a loaded balance, has frequently a material influence on the result of our calculations, in which the error is often multiplied.

D. *Sulphuric Acid.*

I have shown by my earlier experiments that the sulphuret of lead and the sulphate of the protoxide contain sulphur and lead in the same proportions. If now, according to the first of these experiments on the sulphate (C), and according to my former experiments, 100 parts of lead afford

afford 146·33 of sulphate of the protoxide, and according to the experiments on the protoxide (B) 7·7 parts must represent the oxygen of the protoxide, there remain for the sulphuric acid 38·63, which must contain 15·42 of sulphur, that is, as much as combines with 100 parts of lead (A). Consequently if 38·63 parts of sulphuric acid contain 15·42 of sulphur, this acid consists of 39·92 sulphur and 60·08 oxygen. But since in my experiments the quantity of sulphur in the sulphuret of lead is in all probability represented as somewhat too small, the sulphuric acid may possibly contain a very little more sulphur than this proportion.

I shall show, in the second part of this essay, that, to judge from the calculations of the oxygenized carburetted hydrogen gas, of the oxide of carbon, and of sulphuretted hydrogen, a degree of oxidation corresponding to that of the carbonic oxide must be possible for sulphur; and that the sulphur must be to the oxygen very nearly in the proportion of two to one; and I hope to make it probable that this state of oxidation of sulphur is found in the sulphuretted muriatic acid. In this case, 15·42 parts of sulphur must be combined with 7·7 of oxygen. The same quantity of sulphur, with $2 \times 7 \cdot 7$, that is, with 15·4, of oxygen, will consequently constitute the sulphurous acid, and with $3 \times 7 \cdot 7$, or 23·1, the sulphuric. According to this view of the subject, the sulphuric acid must contain in 100 parts 40·03 of sulphur, and 59·97 of oxygen, the sulphurous, 49·968 of sulphur, and 50·032 of oxygen. If we calculate for the sulphate of the protoxide of lead, according to these proportions, we have 14·62 of this sulphate for 10 of lead; and this is precisely the result of the first of my former experiments on this substance.

From this view of the subject we may derive another mode of computing the composition of the sulphuric acid. We see that the protoxide of lead, which saturates a given quantity of sulphuric acid, contains exactly one-third as much oxygen as the acid; and it must contain exactly half as much oxygen as the sulphurous acid by which it is saturated, since the sulphites, in becoming sulphates by the absorption of oxygen, do not alter their state of neutralisation. Now, since 279 parts of the protoxide of lead saturate 100 of sulphuric acid, and these 279 parts contain 19·95 of oxygen; consequently the sulphuric acid must contain in 100 parts 59·85 of oxygen, which differs only by $\frac{1}{1000}$ from the former determination. Supposing the analysis of the sulphate of the protoxide of lead to be slightly incorrect, and that for instance 100 parts of sulphuric acid

saturate

saturate 279·66 of protoxide, the experiment will perfectly agree with this calculation. The difference of the proportion thus determined for the sulphuric acid from that of 40 : 60 for the sulphur and oxygen is so inconsiderable, that we may safely assume these numbers as the true ones. Hence the quantity of sulphuric acid required for saturating a given base may be found by multiplying its oxygen by 5; and that of the sulphurous acid, by multiplying the oxygen of the base by 4.

We cannot expect to obtain complete accuracy in these analyses until we shall have ascertained, by a very accurate comparison of the specific gravities of oxygen gas and sulphurous acid gas, the exact component parts of the latter, as in the case of the carbonic acid.

[To be continued.]

LIV. *Account of a Meteor seen at London and other Places on the Night of Monday, March* 22, 1813. *By* Joseph Steevens, *Esq.*

To Mr. Tilloch.

Sir,—As every phænomenon in meteorology, however trivial, furnishes certain facts towards the improvement of that science, I take the opportunity of communicating some particulars relative to one that appeared on Monday night last. Being in the centre of Moorfields at $9^h\ 22^m$, viewing the configurations of the satellites of Jupiter, (which at that time were • • ○ • • nearly) a meteor of the shape of fig. 1. (Plate VII.) presented itself almost in the field of the telescope; its diameter was about 15′, and at first nearly stationary and not very bright; it appeared near the margin of the small thin black cloud from whence it proceeded westward, which was in the direction of its larger end. By the time it had passed through 20°, it had acquired a great brilliancy, at which period it was so much elongated as to occupy a space of 3° or 4°, the head being very much flattened in the front, and the tail terminating in a well defined point forming a very acute isosceles triangle. At its first appearance there were several radiating points projecting from it; but after having proceeded about 30°, the rays in front had formed themselves into globules of light, some of them perfectly unconnected with the meteor, but still driven before it during its whole passage. It proceeded westward, bearing rather to the north, and described

described nearly a right line passing about 3° north of the Pleiades, and became extinct near another black cloud, having passed through a space of about 60°. However, from its proximity to the earth, its apparent place, direction, figure of its path, &c. would vary very materially to spectators only a few miles asunder.

I understand from a person who saw it at Hounslow, that it at first appeared to the east of the zenith, and proceeded due west; and from another, who saw it at Harrow, that it appeared to descend almost perpendicular, and nearly due south: he conceives that its duration was nearly half a minute.

It appeared as viewed from Hackney, by Mr. C. Paroissien, to be due west, and that it came nearly in contact with the apparent horizon before it was extinct. He conceives its apparent diameter to be equal to half that of the moon, and the intensity of the light much greater than that of the moon. Its distance certainly could not be very great, as I distinctly heard a hissing noise, like that of a squib, as also a crackling like that of a cat's back when briskly rubbed with the hand. Several sparks from the back part of its head were detached during its passage. It first appeared (as seen from the point where I was stationed) about 2° north of Jupiter, in a line between him and the star Castor in Gemini, and proceeded in the direction laid down in fig. 2. Before it vanished, its velocity considerably abated, and its brilliancy was very much reduced. Several of the globules of light were much enlarged, less luminous, and had receded to a distance nearly equal to twice the diameter of the meteor; the whole duration was about 3″.

Having been engaged during the day in some experiments on the Croydon canal, which only occupied my attention at intervals, I had an opportunity of observing a variety of changes in the atmosphere. The morning was showery; the wind variable from SW to NW. Barometer 29,6; thermometer 44, at 9 A.M. The middle of the day was more fine, sometimes quite calm, and the sun bright; about two o'clock several dark but thin clouds arose in the south-west, occasionally approaching and receding from each other, and at some times nearly stationary; but on their arrival near the zenith, slight squalls and showers ensued: this continued at intervals for about an hour until six o'clock, when it was perfectly calm along the line of the canal, although the windmill of the grand Surry (and which is 100 feet lower than the bank of the Croydon

Croydon canal) on the south-east bank was in brisk motion with two of the sails partly furled. Clouds now began to form round the whole horizon, of a dark colour, and appearing to indicate thunder; the general mass remained nearly stationary in the horizon, while small thin ones passed the meridian, most of them giving out a few drops of rain; this continued till about a quarter past nine. Most of the clouds near the zenith disappeared, and two or three small meteors or falling stars displayed themselves, by darting nearly perpendicularly downwards.

I am, sir,

Your most obedient humble servant,

Tower Royal, March 27, 1813. JOSEPH STEEVENS.

LV. *Dissertation on the Paintings of the middle Age, and those called Gothic. Extracted from an unpublished Work on Painting, by M.* PAILLOT DE MONTABERT.

[Concluded from p. 178.]

Analysis of the Qualities of the Painters of the middle Age, and their Parallel with those of the most eminent modern Painters.

WE now come to Raphael. Not only was this celebrated genius nearer antiquity, from the time in which he lived; but I am convinced that he formed in the midst of his career his taste and ideas rather upon ancient models which he incessantly studied, than from the influence of the works of his eminent contemporaries. The latter assisted him, it is true, in this imposing execution of *clair-oscur*, and steadiness of pencil which have since constituted the best part of the grand style: but to the ancients and to his predecessors he was indebted for his chaste love of truth, and simple nature: it was to the ancients that he was indebted for that simplicity which charms in his figures and in his dispositions; and above all for that expression which was so much in unison with his great mind. Who can explain his sensations when he designed the animated figures of *Masaccio*, or studied the bas-reliefs and paintings of the ancients; or finally, when he translated into a better language so many images of the painters of preceding centuries, and whose reputation still resounds throughout Italy? This is impossible; but it is beyond a doubt, that what constitutes the difference of succeeding painters is the union of the same qualities which have so long after his

his time constituted the difference between the ancients and the moderns; and it is also beyond a doubt, that the same man who exposed the drawings of Albert Durer in his workshop, collected all those which the paintings since his time could furnish, and which he frequently imitated. We know besides, that this great painter maintained draftsmen, even in Greece, in order to profit by all the models which he thought could be useful. Such therefore was the superiority of this man: he was nourished by those very fruits which we would reject, and found infinite resources in pictures which we can neither appreciate nor bring into use.

I have only cited Raphael, as yet, for the sake of a more striking comparison: nevertheless, every body knows how infinite was the number of the painters of the sixteenth century who were eager to drink at the same fountain. How grand is the reflection thus presented to the mind! It is clear that the art had degenerated, when we no longer follow the excellent models of the middle age, or those of antiquity, and when artists have recourse only to the modern works of the most famous masters of their own century. Indolent artists found it more easy and convenient to march in the steps of the latter, than to go back to more ancient models, which have now perhaps vanished.

Painters previous to the time of Raphael had therefore studied the art by referring to ancient models; but after this great man, they consulted only recent productions; so that, in this art, the order of the elements has been perverted, and there appeared on the earth a new and unnatural style of painting, of which no nation was acquainted; for, if Apelles or Zeuxis were to visit our modern temples and palaces covered with all the works of the art from the *Primatici* to *Solimene* and *Conca**, in spite of all the talent of the painters who filled up this interval, these two Grecian artists would have understood nothing of the style of painting of the three last centuries. I am of opinion that we might here indulge in a crowd of new speculations, by endeavouring to demonstrate the influence of a degraded style of painting over the Christian religion and worship,—an influence recognised by all the followers of paganism; and which Christian priests have not always

* When it is considered that these most famous artists of the Neapolitan school exercised their art in the finest climate of the world, just over the ruins of Portici and Herculaneum, and those of several monuments which they could examine daily; we cannot help being keenly affected with this influence of the schools over the dictates of nature and good sense.

taken

taken into proper consideration. What have we to oppose, in the schools of recent times, to the great and essential qualities which have been perpetuated by the study of the fragments of the ancients? An academic luxury,—an abundance of shadows without substance, a corruption of taste, and an absurd inclination of the human mind: finally, a degradation of the art, which lost its nobleness and its original objects: we ought here to confess, that since the revival of letters under Leo X, the moderns have always been too stupid or infatuated with all the pompous apparatus of new and increasing knowledge. From this time, pride laid down barriers in the schools, which isolated us from antiquity; and notwithstanding the great examples of some admirable men, contempt and a blind attachment to routine exercised full sway. What I here mark as worthy of reproach is rather, as has been shown, the vice of the schools than that of artists in particular, and many painters of undeniable talents permit us to guess how devoutly they would have followed truth and nature: none of them, it is true, had the liberty of profiting by the knowledge which we have since acquired, nor the models which I have alluded to; for this return of good sense was reserved for the artists of the present century.

Before undertaking the analysis of the important qualities which we trace in the productions of the middle age, we ought to attempt a definition of the situation of all those who are occupied in the cultivation of the arts; and we shall soon be convinced that the greater number are guided in their theory, more by habitude, the dicta of authors, the exclamations of would-be amateurs, and the party spirit of the day, than by the effects of philosophy, and of a constant study of nature. But it is not to those who are desirous of constantly imitating and copying that I address these pages.

If all the painters of Europe were at present agreed as to the manner in which they ought to study the ancients both as to style in general, and as to drawing in particular; if we saw them all marching with a uniform pace, and endeavouring to lay hold of the grand maxims of antiquity, which render the arts so durable, it would certainly be very absurd to propose to them as the subjects of their contemplations, the productions of those very ancients impoverished and almost extinguished, and to vamp up certain works of the fifteenth century, which would have disgraced the best days of Greece: but as the innumerable collections of all the modern schools present them with models of so many different and opposite kinds, as authors, amateurs, and

and rich speculators, do not cease to boast in the very same language of the myriads of pictures of all tastes, all styles, and all manners; it is not surprising that amid this confusion which astounds artists more and more, the paintings of antiquity have lost their credit; and those who praise them, only do it when forced to it, and without endeavouring to be acquainted with them. In this state of things we must successively go back to the fundamental principles of the art. If we only derived from the compositions of the middle age, the advantage of better appreciating the fine paintings of the ancients, a considerable profit would be the result, and perhaps some docile artists would be more easily brought back to the true path, when this same painting of recent times, which is less removed from our present schools, would appear to them more estimable than they had hitherto imagined.

I shall now exhibit a succinct analysis of the qualities of the compositions of the middle age, and compare merely the following parts; the arrangement, expression, draperies, concluding with a few words on colouring.

Of the Arrangement or Disposition.

When we attempt to study the disposition in the works of the middle age, we always recognise an emulation of the ancients, and we cannot doubt the respect which they have maintained for the most famous models. Whatever certain amateurs of pictures of genii grouped and arranged academically may say, the noble, simple, and *uniform* disposition of these paintings is owing to the study of bas-reliefs, cameos, and engraved stones, which so many routine authors interdict painters from imitating, as well as from the study of ancient monuments, almost all of which excel in the order of the arrangement, and by delicate calculations which are incomprehensible to vulgar organs. Raphael, as well as other painters of his time, has frequently imitated this fine method; but subsequently the influence of the Florentine style, the obstinate love of novelty, and of extreme variety, which was gradually introduced, altered the exquisitely simple taste of this great man. We see him collecting and sometimes crowding his figures with a difficult art in given spaces: he seemed to think of multiplying his plans more and more by composing with richness; and hence that taste for arrangement which at present deserves the blame of unprejudiced amateurs when they examine some of his paintings; so true it is that simplicity pleases at all seasons, and is always young

and

and graceful. But not only did Raphael, in his best inspirations, and the immortal *Poussin* arrange like the ancients, and like the most excellent painters of the middle age;—the most eminent painters of the present day have followed the same method: the picture so justly celebrated of the Horatii, for which we are indebted to the pencil of the first painter among the moderns, astonishes by the simplicity of arrangement. The pictures of Phædra, of Pyrrhus, of Psyche, Atala, and so many others, which have embellished the public buildings of Paris, received much of their *éclat* on this account: in a word, all the sagacious artists in Europe have added to their reputation by imitating the maxims of the ancients *.

Of the Expression.

Let me be permitted to mention here the character of the figures, before speaking of the action.

We cannot hesitate to recognize, in the greater mosaics, the figures and even the most shapeless sculptures of these times, that noble taste and grave simplicity of ancient Greece, as well as that poetical style which we endeavour to gather from ancient mythology; and notwithstanding the perspective of the extremities of these figures, which hurts an exact geometrical eye, the air of these divine and apostolic heads, their dress, the form and masses of their traits, the wisdom and dignity of their appearance, although lessened by the feebleness of the art—every thing imposes upon criticism; and the Saints so adroitly painted, so carefully represented by the pencil of so many moderns, cannot support these grand comparisons. Let it not be thought strange, if in speaking of an art which so many persons regard as a simple amusement, I boast of that gravity, a little removed from our manners it is true, but which instead of excluding expression permits it to appear with more unity and force; that calm gravity, which among the most an-

* I cannot refrain from remarking here the fine arrangement of a painting which Santa Bartholi has substituted for another, which is almost destroyed, in the picture of the *Nasos*: it represented a boar hunt, and was found by itself upon Mount Cælius near the Colyseum. I mention it, because it exhibits several personages grouped. As to arrangement in the paintings of the middle age, Bosio, who has shown this quality in a great number of Roman sarcophagi, furnishes us also with examples in various paintings. I shall quote among others that of the cemetery of *Santa Calixta*, tome i. p. 467. Another in the same volume, p. 529: two other paintings of the Portico of the Vatican, tome i. p. 229. I shall also refer to *Ciampini*, tome iii. pp. 16 and 17. We may also quote as models of good arrangement, several pictures of the collection of M. Artaud, among others that of Dello, No. 110, which unites several figures.

cient nations was perhaps still more severe, but which is natural to mankind, as we may be convinced among all nations; and as I have myself remarked amidst the barbarous nations of America, Africa, and even of Italy, which I have studied in this respect in these different climates*.

As to the pantomimes which express action, it must be admitted, that since the period of the paintings of the manuscript of Terence in the Vatican, attributed to the time of Constantine, to the most trifling paintings of the same kind which are to be met with, they are clear, natural, and significant. The subjects are understood easily, and at a distance; no useless movements, no equivoques, no forced complications. The signs are not so numerous as to be more striking. What may we not erect upon such simple and solid bases, and what force may we not add to these elements of expression, true science, and the cultivation of drawing? Is it not these very pantomimes, strong by their clearness, and so expressive in point of *naïveté*, which still constitute the glory of Raphael, Poussin, and the greatest painters of our days? It is useless to recall here those heads full of life which have excited the admiration of the critics. But even if we should not have our own eyes as judges, could we withstand the sentiments of some writers who have been struck with the expression of the painters of these times, and among others with the testimony of St. Gregory Nazianzen, who informs us that he never cast his eyes on a picture in which was represented the sacrifice of Isaac, without being violently moved and without shedding tears,—so well can painting pourtray this tender scene! Finally, from the pious resignation of the virgins and martyrs—from the ferocious image of the executioner, to the chaste and ingenuous grace of the *Mater Dei*; these paintings afford us constant food for meditation and study, and may pave the way for important reforms in the arts†.

Of their Draperies.

What shall we say of the draperies which still do honour to the arts of the ancients? Shall I here recall what

* On the subject of the character of figures, we may consult *Ciampini*. tome ii. tab. liv. as well as the *mosaic* of Saint Agatha of Ravenna, already quoted, tome i. tab. xlv. These paintings call to recollection the riches and simplicity of the figures of the Greek vases, and all the noble grace of eastern imagery.

† There is a crowd of works which contain engravings after very fine monuments, and which might serve as a proof: but I content myself with quoting Bosio, in the work of which, besides the Sarcophagi which frequently retrace the remarkable expression of the Shepherds adoring the Messiah,

what all the world has observed? I wish to speak of that trivial corruption of taste in this particular work to be ascertained in the succeeding schools, who abandoned that fine arrangement which we admire in ancient dresses, and which have been preserved down to the time of Raphael, when so many artists were influenced both by the ridiculous usages of the habits and eccenrric formed stuffs of those times; and by the *mannerism* introduced by some rash master; a character which was so easy of imitation, and to which we owe that enormous heap of stuffs, and those barbarous adjustments which are insupportable to the sight. I ought to add here, that the art of the most eminent painters of our days is still related in this respect with antiquity: and notwithstanding the respect which we owe to the Carracci, to Guido, who have been constantly imitated and praised in this respect, who has not remarked how much success modern art has obtained by this single reform? And who does not prefer the taste of the costume received in our best paintings to those conventional and shocking *lazzi* with which a depraved taste loads our most famous pictures? It is proper to add here, that the veneration for the schools of Italy still propagates doubts, that the writers who have determined the limits of the two arts of sculpture and painting have gone too far, and have exaggerated the demarcations in order to justify so many celebrated painters: finally, that the best method of fixing our ideas on the subject of the draperies in our art, is to contemplate the examples left by the ancients, and to meditate upon the effects still exhibited in this respect by the paintings of the middle age *; and it is so very true that we have few things to change in painting, in the imitation of the draperies of ancient sculpture, that in the decorations in which the painters are liberated from the trammels of the school, and where they have literally translated the ancients, these same draperies bear an excellent character not-

Messiah, that of the Virgin and the Infant Jesus, or the *naïveté* of the young persons who throw draperies under the footsteps of our Saviour when entering Nazareth, we find several paintings remarkable for a sage and well reasoned expression. See that of the Cemetery of Saint Priscilla, tome ii. p. 311, representing Isaac carrying the wood for his own sacrifice: Abraham on the point of immolating him, tome ii. p. 87: the Martyrdom of Saint Sebastian, tome ii. p. 325, as well as another painting excellent in point of expression, tome ii. p. 211, no. 7.

* These models are not rare; but see among others the draperies of a painting of the Cemeteries of Pontica and S. Abdon. Bosio, tome i. p. 385; and another of the Cemetery of St. Julius in the same volume, p. 354: he even finds some very fine in the works of Perugin; and, in a word, in all those which were made before the manner of the Florentine school.

withstanding

withstanding their ruggedness and want of truth in the execution.

It is totally useless to speak here of the ornaments of the paintings of the middle age: the unanimity of opinion on the delicate taste of those models which are perpetuated without mixture renders this analysis unnecessary. I shall now conclude by a few reflections on their colouring.

On their Colouring.

I shall not dwell long upon the colouring of the paintings of the middle age. I shall only remark, that the crudity and discordance of the colours are much less revolting when the whole system of colouring is lively and luminous, like that which was employed in those times, than when the colours are dull and heavy like those used with oil. This reflection may involve a question on the subject of colouring, which it is unnecessary to explain here. I merely throw out the idea, to diminish the aversion which those have for brisk and entire colours who do not take any but oil paintings into the comparison. To conclude: the fine Guido of Sienna of the height of six feet, which is to be seen in the cabinet of M. Artaud, is painted in a most delicate tone, and with that *commixtura colorum* of Pliny which brings to our recollection the best schools of the ancients.

I have endeavoured in another Essay*, by quoting some pages from the second edition of M. Artaud's work, to prove that painting in oil had deprived the art of its *naïveté* of colouring. I have attempted to show the inconveniences of this painting so much spoken of, and which was probably known and rejected by several nations on account of its interminable obscurity, and of which latterly John of Bruges was unable to foresee the slow carbonisation. I shall not here repeat the ideas which I then hazarded; but I shall content myself with saying, that I am convinced that the restoration of a more natural and true process may have a very important influence upon the arts by the analogy of expressed truths: full of these ideas, and guided by the desire of being useful, I have made constant efforts to recover the material painting of the ancients. I hope that the experiments which I purpose to make known, will determine all unprejudiced artists to employ the processes

* Considérations sur l'Etat de la Peinture en Italie dans les quatre Siècles qui ont précédé celui de Raphael. In 8vo. Paris. Schœll. 1811.

of an unalterable and easy description, and which can hand down to posterity the glory and genius of our artists *.

I think I have demonstrated that the principal parts of painting were preserved in the middle age, and that as soon as the art of designing had lost its strength and accuracy, the *claire-oscure* was almost forgotten, the art of colouring very little cultivated, and the execution very often despicable: what remained nevertheless formed those qualities which were most difficult to recover among altered manners, qualities grand and simple, which constitute the character and dignity of the art, and the loss of which the boldness of our most intrepid artists can never repair.

Conclusion.

I conclude from all these observations, that the paintings of the middle age are the records of the precious doctrines of ancient art; that they are not vitiated, and that they ought not to be confounded with some barbarous and *mannered* works painted during the sixteenth and seventeenth centuries in the north of Europe; that they have formed our greatest painters; and that those only have a right to neglect them, who have attained the climax of the best models of antiquity:—in a word, that artists ought to observe and study them without intermission, and as easy versions, calculated to explain the secret idioms of a language which is of most difficult attainment.

* It ought to be remarked, that most of the paintings of the middle age, existing in cabinets, have very rarely preserved their primitive colours, considering the practice of reviving them by means of varnish. I shall not here speak of all the ravages or decompositions which may result from this method, when indiscriminately employed upon paintings the materials of which have not been previously studied. In general, it is very rare to find paintings, either antique or Gothic, which are really originals (*vierges*), and which do not exhibit some alterations proceeding from restorations.

It seems that the famous painting of Colantonio, dated 1436, which is preserved at Naples, was the occasion of so many disputes, only because it was afterwards covered, like many others, by a slight coat of oil: the same perhaps may be said of those contained in the Gallery at Vienna: one is dated in 1090, the other in 1292. This last is the work of Thomas Mutina, a Bohemian gentleman. Some others of the same gallery are dated in the middle of the fourteenth century, and are by Theodoric of Prague and Nicholas Wurmser of Strasburg. Now John of Bruges died about the middle of the fifteenth century, in 1441. Upon the whole, without having recourse to numerous works upon this subject, it is scarcely credible that the use of oil in painting had never been imagined before the existence of that celebrated Fleming, who being a chemist could perhaps put in practice ancient recipes, the principles of which were well known.

LVI. *On an Equation in* LAPLACE's "Méchanique Céleste."

To Mr. Tilloch.

SIR,—HAVING observed in the Philosophical Magazine for last January, a communication from Mr. White, respecting an equation in the *Méchanique Céleste* of Laplace, I am induced to send you this letter concerning what appears to me to be an oversight in the author of that most admirable work. If Mr. White, or any other of your learned correspondents, would favour me with his opinion respecting it, I should esteem it as a particular favour. In vol. i. page 57, of the *Méchanique Céleste*, Laplace supposes that $c = \Sigma . m . \frac{(xdy - ydx)}{df}$, which he says, page 63 of the same volume, may be put into the following form:

$c . \Sigma . m = \Sigma . m\overset{\prime}{m} . \left\{ \frac{(\overset{\prime}{x} - x)(d\overset{\prime}{y} - dy) - (\overset{\prime}{y} - y)(d\overset{\prime}{x} - dx)}{df} \right\}$; Now, at page 130 of the same volume, he says that the equation const. $= \Sigma . m . \frac{(xdy - ydx)}{df} - \frac{\Sigma . mx}{M + \Sigma . m} . \Sigma . m . \frac{dy}{df} + \frac{\Sigma . my}{M + \Sigma . m} . \Sigma . m . \frac{dx}{df}$; if multiplied by $M + \Sigma . m$ will be changed into the following:

$$c = M . \Sigma . \frac{(xdy - ydx)}{df} + \Sigma . m\overset{\prime}{m} . \left\{ \frac{(\overset{\prime}{x} - x)(d\overset{\prime}{y} - dy) - (\overset{\prime}{y} - y)(d\overset{\prime}{x} - dx)}{df} \right\}.$$

It appears to me from an investigation of these expressions, that in the first instance he makes $\Sigma . m . \Sigma . m . \frac{(xdy - ydx)}{df}$ equal to $\Sigma . m\overset{\prime}{m} . \left\{ \frac{(\overset{\prime}{x} - x)(d\overset{\prime}{y} - dy) - (\overset{\prime}{y} - y)(d\overset{\prime}{x} - dx)}{df} \right\}$, and in the second he makes $\Sigma . m . \Sigma . m . \frac{(xdy - ydx)}{df} - \Sigma . mx . \Sigma . m . \frac{dy}{df} + \Sigma . my . \Sigma . m . \frac{dx}{df}$ equal to the same quantity; which is impossible, unless the two last terms of the last equation be equal to nothing; which is not supposed to be the case.

I remain, sir,

Yours respectfully,

JOHN THOMSON.

LVII. *Case of Hydrophobia cured in India by Bleeding, By* John Shoolbred, *M.D. From the Supplement to the Calcutta Government Gazette, June* 8, 1812 *.

Tuesday, May 5, 1812.—About 3 P.M. Ameir, a Muselman Bhestie, from 25 to 30 years of age, and middle stature, in the service of Mr. John Wood, schoolmaster, at Chowringhee, was brought to the Native Hospital, labouring under the most unequivocal symptoms of hydrophobia. The note from Mr. Wood, requesting admission for this patient, and the friends who accompanied him, stated that he had been bitten in the leg about three weeks before, by a dog believed to be mad, and that the symptoms of his disease had appeared that morning, the 5th.

I visited him in the hospital, the moment I heard of his arrival, and found him sitting on the side of a cot, with an attendant holding him by each arm. The first view was sufficient to satisfy me of the nature of his complaint. His body, arms, and throat were affected with constant and uncontrolable spasmodic startings. The muscles were thrown into quick convulsive action at each inspiration, drawing back the angles of the mouth, and at the same instant depressing the lower jaw, so as to communicate the most hideous expression to the countenance. His eyes appeared starting from their sockets and suffused with blood; sometimes fixed in a wild and terrific stare; at others, rolling about, as if they followed some ideal object of terror, from which he apprehended immediate danger. A viscid saliva flowed from his mouth, which was always open, except when the lips were momentarily brought together for the purpose of forcibly expelling the offensive secretion that adhered to them, and which he effected with that peculiar kind of noise, which has been often compared to the barking of a dog. His temples and throat were bedewed with clammy moisture. His respiration was exceedingly hurried, and might more properly be called panting than breathing; or, it still more nearly resembled that short and uninterrupted kind of sobbing, that takes place when a person gradually descends into the cold bath. He was exceedingly impatient of restraint, and whenever he could get a hand disengaged, he immediately struck the pit of his stomach with it—pointing out that part as the seat of some undescribable uneasiness. From the constant agitation of his whole frame and the startings of his arms, it was impossible

* Our readers will recollect that a few months ago we briefly noticed this remarkable case.—Edit.

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to count his pulse with exactness; it was, however, very unequal, both in strength and frequency; at times scarcely perceptible, and then rising again under the finger; sometimes moderately slow and regular for a few pulsations, and immediately after, so quick as not to be counted; but conveying, upon the whole, an idea of a greatly oppressed and impeded circulation. His skin was not hot; and though his head was in incessant motion, accompanied with such savage expression and contortion of countenance as might easily have alarmed those unaccustomed to such appearances, he made no attempt to bite; which is far from being a frequent symptom of the disease, and, when it does occur, must be considered merely as an act of impatience at being held—and no more than the peculiar noise above noticed, as indicating any thing of the canine nature imparted by the bite, an opinion which has been sometimes fancifully but absurdly entertained.

When questioned concerning his own feelings, or the cause of his illness, he was incapable of making any reply; being prevented, it is probable, either by the hurried state of his respiration, or by his mind being too deeply absorbed in the contemplation of horrible ideas, to admit of his attending to the queries addressed to him.

I desired water to be offered to him; at the mention of which he started with increased horror and agitation, and endeavoured to disengage himself from those that held him. When one of the attendants approached with a cup of water, he looked at it wishfully, and after some efforts, with apparent reluctance, stretched out his hand to take hold of it; but before he could reach the cup, his hand was suddenly drawn back by a convulsive motion: at the same instant he turned away his head, and writhed himself round on the bed in an agony of terror and despair, wholly inconceivable by any person who has not been a witness of the horrors of this most dreadful, and hitherto, it may be added, most irremediable of human maladies.

Such was the state of the patient at the moment of his admission, and for the few minutes that necessarily elapsed while these appearances were passing under my observation.

Of the nature of the complaint there could not exist a shadow of doubt; and having so recently read in the Madras papers a case of hydrophobia successfully treated by Mr. Tymon, of His Majesty's 22d dragoons, by bleeding, mercury, and opium, I determined on the immediate adoption of the same plan.

 I there-

I therefore without delay opened a vein in the right arm by a large orifice, out of which the blood sprung with uncommon impetuosity, and of so florid a colour as to resemble arterial rather than venous blood. By the time that sixteen or twenty ounces of blood had flowed, the spasmodic startings of his arms, body, and neck had considerably diminished, his breathing had become more calm, with less contortion of countenance, and he audibly acknowledged that the pain about the præcordia and region of the stomach was upon the decline. Encouraged by these incipient appearances of amendment, I allowed the flow of blood to continue; and when about two pints were taken away, seeing him greatly composed, I desired water to be again offered to him—when, equally to my astonishment and delight, he took the cup in his left hand, the blood still flowing from the right arm, and calmly—but with indescribable expression of satisfaction, drank two or three ounces of water—the sight of which but a few minutes before had thrown him into the most dreadful agonies. Soon after swallowing the water, he retched three or four times, but ejected nothing but saliva from his mouth and fauces; and finding now that his pulse was 104, weak, soft, and regular; that he was become faint, and that all appearance of uneasiness had ceased, so as to allow him to take a second draught of water, about four ounces, I closed the vein and laid him down on the bed. At this moment he expressed a desire to have a natural alvine evacuation, and wished to go out of the hospital for that purpose; but as that could not be complied with, he took no more notice of it at this time. It is worthy of remark also, that during the bleeding he made a sign to have himself fanned, a thing I never knew a hydrophobic patient to do before;—their distress being so uniformly increased by any current of air blowing upon them, that, according to all my experience, the dread of air in motion is as constant an attendant on the disease as the dread of water itself.

After the bleeding he remained perfectly quiet, and fell into a slumber for about an hour;—another circumstance which also strongly marks the abolition of the disease, as no hydrophobic patient was ever known to sleep. When he awoke, he expressed a wish to have some sherbet; which was immediately given to him, and he drank four ounces of it with perfect ease. He then fell into another slumber, during which some convulsive startings were again perceptible about his arms, chest, and face, but not strong enough to wake him. At a quarter past five he spontaneously

ously awoke, and appeared again somewhat agitated, with more suspicion in his looks, and of apparent doubt whether he could swallow as well as before; for when he took the cup, he put it to his lips with a quick motion, and gulped down about four ounces of water in a hurried manner, as if afraid that the difficulty of swallowing would be increased by a moment's delay. He also put his hand to the region of the stomach, and said that the pain in that part was returning. These threatening appearances of relapse determined me to hazard a further detraction of blood. I therefore immediately opened a vein in the left arm, and allowed the blood to flow again till he completely fainted; but previously to this effect of the bleeding, the pain at the stomach had ceased; and while the blood was yet flowing he had again drunk four ounces of water without fear or disgust. When he recovered from the fainting fit, he retched several times, but, as before, discharged nothing but saliva.

At the end of the first bleeding his pulse was 104; immediately before the second, it was 96, with a slight degree of sharpness in the beat; and after recovering from the fainting occasioned by the second bleeding, it was 88, regular, soft, and feeble, and he now complained of nothing but extreme weakness, and giddiness of the head. And at this stage of the case, I apprehend, it will be allowed that the cure of the hydrophobia was complete—whether it would be permanent or not, remained yet to be seen.

When I began the treatment of this patient, it was my intention, as I have said, to follow in every circumstance the practice pursued in Mr. Tymon's successful case; and accordingly, a draught with 100 drops of tincture of opium, and an enema of 300, were in readiness to be administered immediately after the bleeding. But seeing the surprising effects of the bleeding alone, and feeling convinced that the disease was, for the present at least, completely annihilated by the copiousness of that evacuation, I determined to preserve the treatment as simple as possible, in order that, if the patient did finally recover, it might with certainty be known to what he owed his safety; and that thence the application of the same practice to future cases of hydrophobia might with the greater confidence be recommended:—a resolution in which I was the more confirmed, from having heard some medical friends, whose opinions are entitled to every degree of respect, ascribe Mr. Tymon's success to the mercury he had used, rather than to the bleeding.

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I am now fully persuaded, however, that I might safely, as far as the hydrophobia was concerned, have omitted all remedies after the bleeding; but thinking that calomel and opium in repeated doses were more likely than any thing else, to induce that state of the system which would be least favourable to a relapse; and also that if the patient, notwithstanding his present promising appearance, did not finally recover, it would certainly be said that I had not given him a fair chance, by departing in any particular from the treatment which had proved so successful in the hands of Mr. Tymon, I was inclined to conform to it so far, as to order four grains of calomel and one grain of opium to be given every three hours.

The first pill was taken at a quarter before six; but it was immediately rejected, followed by some water. A second was given five minutes before six, and remained. He now slept till seven—then drank some more water, and had a natural evacuation of his bowels;—another circumstance which confirmed me in the belief that the disease was completely and permanently subdued—having never before seen in my own experience, nor read in any history of the disease, of such an occurrence as a natural action of the alimentary canal in a case of hydrophobia.

At nine he took another pill, and again at twelve—and continued to slumber and drink water as often as he pleased.

Wednesday, May 6th—(2d day) six A.M. Has passed the night well. Took a pill at three, and another now. Has drunk water frequently. Pulse 84. Skin cool. Tongue clean at the edges—some remains of betel, eaten before he was taken ill, covered the centre part. Two more alvine evacuations during the night. Complains of head-ache—but is entirely free from uneasiness about the stomach.

On examining the blood drawn yesterday, it is found not to be in the least convex—neither does it exhibit the slightest appearance of what is called the buffy coat. The quantity first drawn, making allowance for the evaporation of the night, measures 40 ounces; and the last between seven and eight.

Nine A.M.—Took another pill, which was followed by another evacuation; and in half an hour afterwards he ate eight ounces of sago. Is quite composed, and can answer questions distinctly concerning the accident and subsequent occurrences, till the time he was taken ill.

He says that 19 days ago (including this day) when returning about four in the afternoon, from his own house at Russa-

Russapuglah, to his master's at Chowringhee, he saw a pariah dog seize a fisherman, and bite him. Several people were collected at the spot—he also approached, when the same dog ran at him, and as he was retreating before him, bit him in the back part of the right leg, about six inches above the ankle, where he shows two scars at the distance of an inch and a half from each other, but without any appearance of inflammation or thickening of the integuments. The dog, after biting him, disappeared, and he does not know what became of him or of the fisherman. The wounds bled a good deal; but not being very deep, they soon healed without any application. He took no remedy, except, on the day he was bitten, a small piece of scarlet cloth (*sooltanee banat*) wrapt up in a piece of ripe plantain, which was recommended to him as an infallible antidote against infection from the bite of a mad dog. He never saw any one in hydrophobia; and though he had heard that persons bitten by a mad dog were liable to such a disease, the apprehension of it never dwelt on his mind, or scarcely ever occurred to him after the day on which he was bitten. He continued in his usual health till the 4th instant, seventeen days after the bite, when he found himself dull, heavy, and listless, with loss of appetite and frequent apprehension that dogs, cats, and jackalls were about to seize upon him. He also felt a pricking sensation in the part bitten. When his mother-in-law brought him his breakfast, he was afraid to eat it. He continued his business, however, of taking water from the tank to the house, till about noon of that day, after which he could not bear to look on or to touch the water, being constantly harassed, whenever he attempted to do so, with the horrible appearance of different animals ready to devour him. He now, for the first time, thought of the disease arising from the bite of a mad dog, was convinced that was the cause of his present distress, and fully believed that he should die of it. He ate no supper, nor drank any water that night, in consequence of the horrible phantoms that incessantly haunted his imagination. In the morning, all his horrors were increased, the spasms came on, accompanied by anxiety, oppression, and pain about the præcordia and stomach; and those about him say that he continued to get worse in every respect, until he arrived at the hospital in the state already described. He does not himself distinctly remember any thing that happened during the whole day. He has some faint recollection of having been at his own house; but how he got there—when he left it—or by what means he was brought to

to the hospital, he does not at all know. The first thing he can recall to his mind is drinking the sherbet—and he says he has had his senses perfectly since that time—and that all his fears then left him, and have not since returned. This however is not entirely correct, as he acknowledges that he does not recollect the second bleeding, which shows that the disease had then so far returned as again to disorder his mental faculties.

Half past ten A.M.—Complains of severe head-ache, and his eyes are more suffused than they were in the morning. No return of other symptoms.

Head shaved, and six leeches applied to each temple.

Three P.M.—Took a pill at twelve, and another just now. Leeches bled freely. Head-ache relieved. Took eight ounces more of sago about noon.

Six P.M.—The same. Has now taken 28 grs. of calomel and seven of opium. To take from this time only two grs. of calomel and half a grain of opium every three hours.

Nine P.M.—Has slept for two hours. Pulse 80. Took another of the pills last ordered; also some more sago. Copious bilious evacuation. Still complains of giddiness, but not head-ache.

Thursday, the 7th, (3d day,) six A.M.—Took a pill at twelve, but refused one at three, saying his mouth was sore. Took one now. Has been rather restless in the night. Threw up some bile this morning.

Ten A.M.—Exceedingly distressed with excessive secretion of bile, which he is frequently throwing up and also passing downwards in great quantity; and of a dark green colour. Pulse 110. Some heat of skin—expression of uneasiness in his countenance—burning sensation all over the abdomen; but quite different, he says, from the former pain about the stomach. He was ordered a pint of infusion of camomile, which brought off much bile. At eleven, eight grains of calomel, and at half past twelve, half a dram each of jalap and magnesia. From the effects of these remedies, he was much relieved in the evening; though the complaint continued to disturb him in the night, and it was necessary on

Friday morning the 8th, (4th day,) to promote the further evacuation of bile by senna, manna, and cream of tartar; and to order an enema of conjee to allay local irritation. Pulse only 80, soft. Burning removed from the abdomen. Ate a water melon in the night. Copious flow of saliva from his mouth.

Saturday 9th, (5th day,) nine A.M.—Has passed a good night.

night. Excessive secretion of bile has ceased. Clamorous for food—but I allow him only rice and sago—declines milk. He appears now to be free from all complaint. After this time nothing remarkable occurred. He had a strong appetite, and was allowed vegetable curry. For several evenings some heat of skin and acceleration of pulse were perceptible; but these soon went off, from cold bathing and a constant attention to keep his bowels in an open state.

Monday, May 18th, (14th day.)—Has been for some days past on the usual hospital diet—and feeling himself well in every respect, now expresses a wish to be discharged and return to his usual business; but as the weather is exceedingly hot (thermometer in the shade from 95° to 100°), I have prevailed upon him to continue in the hospital till the setting in of the rains.—I shall then, if possible, persuade him to remain in my own employment for the next twelve months; lest, if he were discharged, and should happen to die of whatever disease, it might be alleged that he was after all carried off by a relapse of the hydrophobia.

[To be continued.]

LVIII. *Notices respecting New Books.*

Elements of Crystallography, after the Method of Hauy, *with or without a Series of geometrical Models, both solid and dissected, exhibiting the Forms of Crystals, their geometrical Structure, Dissections, and general Laws according to which the immense Variety of actually existing Crystals are produced. By* Fredrick Accum, *Operative Chemist, Lecturer on Practical Chemistry, &c. M. R. I. A., F. L. S. &c.* pp. lxiv. and 396, 8vo. with four Plates. Longman and Co. 1813.

The author has very laudably endeavoured to render the difficult subject of crystallography familiar to persons unacquainted with geometry. The task was somewhat arduous; but those who cannot comprehend his figures and descriptions, may have recourse to his models, which supersede the necessity of mathematical knowledge. His models amount to fifty, his figures to 103, which embrace nearly all the different forms of crystalline bodies. As crystallography is rather a new science, the necessity of elementary or introductory treatises on it must be obvious. Hitherto nothing of the kind has appeared either in France or

or England, and this circumstance has very materially obstructed the progress of this interesting study. Some Italian and Spanish professors have published brief outlines of it, designed to facilitate its comprehension by their pupils; but a complete and elementary treatise on crystallography is yet a desideratum in literature. Mr. A. might have rendered this work still more useful, by devoting a chapter to illustrate or rather to exhibit in a concise manner the mathematical method adopted by Haüy. Without some such exhibition his work does great injustice to the illustrious author of the crystallographic system; it degrades the discoveries and inventions of Haüy from the exalted rank of a science, to that of a mere mechanical process of measuring solids and angles. It is true, his plan is more simple; but should his readers wish to consult the original *Traité de Minéralogie*, the surprise may perhaps deter them altogether from pursuing the study further. The object of an elementary work is to open the door to knowledge, not to find a lazy substitute for it. But perhaps we should rather be thankful to the author for what he has done, than blame him for what he has left undone; a full knowledge of mineral architecture is not to be acquired at once.

Mr. Accum commences with a definition of the term crystal, the growth of crystals, and the ingenious opinion of Dr. Young that crystallization is the universal cause of solidity. The conditions of crystallizing bodies, and the various causes which influence the process of crystallization, are so intimately connected with chemistry, and still so inadequately known, that it would require much time and labour to develop them with any precision. A few extracts will convey an idea of the author's popular manner of illustrating this part of his subject, which is well adapted for exhibition in lectures to a mixed audience.

Crystallization by reduction of temperature. "If we melt a ladle full of bismuth, antimony, zinc, sulphur, or muriate of lead, and allow it to cool slowly and quietly till a thin crust has formed on the surface, and then by means of a pointed iron make two small opposite apertures through the crust, and quickly pour out by one, the fluid portion as carefully and with as little motion of the mass as possible, whilst the air enters by the other aperture, there will appear on removing the upper crust with a chisel, when the vessel is cold, a cup-shaped concavity studded with crystals, very brilliant, and more or less regular, according to the magnitude of the mass employed, the tranquillity and slowness with which it has cooled, and the dexterity with which

which the fluid central portion, at the moment before it commenced to solidify, was decanted from the crystallized part." Water also crystallizes by the abstraction of caloric, and snow is often found crystallized in stellæ with six radii. An English university professor of mineralogy observed this at Petersburg, and gave a drawing of it in an account of his Travels, as if it had been a new discovery, although Dr. Hooke many years ago published similar figures.

Benzoic acid furnishes a familiar example of crystallization effected by sublimation, or the application of heat. The instances of crystallization produced by chemical affinity are very numerous. The most simple and easy experiment of this nature is, by adding highly rectified spirits to aqueous solutions of the salts, when the spirits and water unite, and the salts immediately resume their crystalline state. The crystallizations of silver, lead, zinc, &c. are now become nursery amusements. Large and perfect crystals, however, are still rarely produced by art. Mr. Sims, of Norwich, has been more successful in obtaining curious and magnificent crystals, than any other chemist of the age. Time, space, repose, light and air, are necessary to crystallization. The effect of light on solutions of muriate of ammonia and prussiate of potash, when placed to crystallize, is curious, and tends to prove the materiality of light. The crystallization of those salts may be directed at pleasure by the introduction of light at one side or another of the vessels containing their solutions. Camphor displays the same affinity for light. The electricity of crystals is also noticed by Mr. A. The principal crystals which become electric merely by heat are borate of magnesia, Brazilian topaz, tourmalin, prehnite (erroneously printed phrenite throughout the volume), crystallized oxide of zinc, siberite, lepidolite, and kaupolite.

The figures in this work (wood-cuts) are tolerably accurate, and the explanations are remarkably simple, and easily comprehended. These advantages we should think fully sufficient to recommend it to public attention. It has evidently cost the author much labour and expense, for which he can receive no adequate remuneration, except that of public approbation. He has very properly annexed the classification of minerals introduced by Haüy, and generally adopted by French mineralogists.

Capt. Laskey has in the press, a scientific Description of the Rarities in that magnificent collection "The Hunterian Museum," now deposited at the College of Glasgow. It is intended

intended to comprise the rare, curious, and valuable articles in every department of Art, Science, and Literature contained in that great Repository. This work, so generally interesting, may be expected to appear early in July, when we have no doubt it will be received with the favour so acceptable an offering deserves.

Mr. Thomas Forster has in the press, Researches concerning atmospheric Phænomena, in one volume, 8vo.

Mr. Bakewell's Introduction to Geology will appear early in June.

Professor Leslie, of Edinburgh, has in the press a valuable work "On the Relations of Air to Heat and Moisture."

LIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

April 29.—THE Society met after the holidays. The Earl of Morton in the chair; when the reading of a paper on the Alcohol of Sulphur, or Sulphuret of Carbon, by Professor Berzelius of Stockholm, and Dr. Marcet of London, was read, which, together with an Appendix by Professor Berzelius, occupied three successive meetings of the Society.

The series of experiments related in this paper were performed in the months of July, August, and September last, during Professor Berzelius's stay in this country, and the leading points of the inquiry were then ascertained. The singular oily liquid which is the object of these experiments was discovered in 1796 by Lampadius, and has since been the subject of much speculation and experimental controversy. Indeed there are few substances the analysis of which has given rise to so much diversity of opinion, as the alcohol of sulphur. Lampadius believed it to be a compound of sulphur and hydrogen. Clement and Desormes considered it as a combination of sulphur and charcoal; Berthollet, as a triple compound of sulphur, charcoal, and hydrogen; and Berthollet junior, as well as Davy, adopted the opinion of Lampadius. In France, very recently, Mr. Cluzel, from an elaborate series of experiments, concluded that the alcohol of sulphur consisted of sulphur, carbon, hydrogen, and azote; but Messrs. Berthollet, Thenard, and Vauquelin, the reporters of Mr. Cluzel's inquiry, having made some experiments of their own upon the subject, concluded that the liquid in question was a compound of

of sulphur and carbon only, a result which agrees perfectly with that of the paper before us.

The alcohol of sulphur, when rectified by distillation, is a perfectly transparent fluid, which is insoluble in water, and has great refractive powers and considerable specific weight. It is exceedingly volatile, more so even than ether; is highly inflammable, is capable of dissolving phosphorus and sulphur, and is itself soluble in alcohol and ether. It combines with the new discovered detonating oily compound without exploding, even if phosphorus or oil be present and heat applied.

The authors ascertained the chemical nature of the alcohol of sulphur by various methods. By exploding it, in a state of vapour, with oxygen gas, sulphureous acid gas and carbonic acid gas are formed, without any production of water. From this, and various other experiments, the absence of hydrogen was proved, and the presence of carbon ascertained. But the process by which the proportion of sulphur and carbon in this compound was ascertained, consisted in causing a known quantity of the alcohol of sulphur, in vapour, to pass through red hot oxide of iron. The oily liquid was thus resolved into sulphuret of iron, sulphureous acid gas and carbonic acid gas; and by a careful examination of these products, the authors were enabled to conclude, that the alcohol of sulphur was composed of about 85 parts of sulphur to 15 of carbon, which, in Mr. Dalton's mode of expressing proportions, correspond to two atoms of sulphur to one of carbon.

May 20.—Earl Morton in the chair. A paper was read describing a newly-invented lamp, designed to be used in coal-mines, and to prevent the dreadful explosions of carburetted hydrogen gas, which are still so common and so destructive, notwithstanding the advantages of ventilation. The description would not be intelligible without a drawing; but the principle was merely that of completely isolating the lamp from the atmosphere, enveloping it in a large globe, surrounding the base of the burner with water, and conveying the atmosphere of the mine to it, by means of a pair of common bellows, to support combustion. A lamp so situated could never be affected by any sudden current of inflammable gas, and would answer every purpose of affording light to the workmen. It appears that during the last seven years above 100 miners have been killed in the county of Durham only by explosions, leaving above 300 women and children to be supported by the public.

⁎ The name of the female with the black arm, described by Dr. Wells, and mentioned in last report, was H. West and not Trest.

LINNEAN SOCIETY.

On Monday last, the Anniversary Meeting of the Linnean Society of London was held at the Society's house in Gerrard-street, Soho, for the Election of a Council and Officers for the present year, when the following Members were declared to be of the Council, viz.

James Edward Smith, M.D.
George Anderson, Esq.
John Barrow, Esq.
Samuel, Lord Bishop of Carlisle.
Sir Thomas G. Cullum, Bart.
Philip Derbishire, Esq.
Mr. James Dickson.
Charles Konig, Esq.
Aylmer Bourke Lambert, Esq.
Alexander MacLeay, Esq.
Thomas Marsham, Esq.
Wm. George Maton, M.D.
Rev. Thomas Rackett.
John Sims, M.D.
Edward, Lord Stanley.

And the following were declared to be the Officers for the present year, viz.

James Edward Smith, M.D. President.
Samuel, Lord Bishop of Carlisle,
Aylmer Bourke Lambert, Esq.
Thomas Marsham, Esq.
William George Maton, M.D. } Vice Presidents.
Thomas Marsham, Esq. Treasurer.
Alexander MacLeay, Esq.
Mr. Richard Taylor, } Secretaries.

The Members of the Society afterwards dined together at the Freemasons' Tavern, Great Queen-street, according to annual custom.

GEOLOGICAL SOCIETY.

May 7, 1813.—The President in the chair.

Matthew Cully, Esq. of Askeld, Northumberland,
Thomas Brandram, Esq. of Lee, Kent,

were severally elected members of the Society.

The reading of Dr. MacCulloch's paper on the Geology of certain Parts of Scotland was begun.

The first article in this paper treats of the granular quartz rock of the island of Jura. This, by some denominated granite, and by others granular quartz, but by all who have hitherto described it considered as a primitive rock, constitutes

constitutes the principal and fundamental rock of the island: in particular, the three well known conical *paps of Jura*, of the height of 2500 or 2600 feet, are entirely composed of this mineral. It is disposed in regular uninterrupted strata six or eight feet in thickness, and rising for the most part at a considerable angle towards the west. These strata do not appear to be traversed by veins, except of quartz, nor do they alternate with any other rock. On the shore, however, the dip and direction of the beds vary considerably. The mineralogical composition of this rock presents several varieties. Sometimes it is extremely compact, being made up of grains of quartz of various degrees of magnitude united without cement. Sometimes besides the quartz it contains felspar, seemingly in rounded fragments, and often decomposed into clay.

In one specimen a manifestly water-worn pebble of quartz is inclosed: and upon the whole the rock may be considered a kind of sandstone consisting of quartz and felspar, the former in the larger proportion. In some of the beds the sandstone passes into grauwacke slate by mixture with pieces of mica slate.

From these circumstances Dr. M. considers the quartz rock of Jura as a mechanical deposit formed from the fragments of older ones, and not as belonging to the Wernerian primitive class. According to Professor Jameson, however, this very rock rises from below the micaceous schistus. We must therefore admit, either that the micaceous schistus described by Professor J. is not primitive, or that the circumstances under which the primitive rocks were formed were such as to exclude at the same time the production of a mixed mechanical deposition.

The next article in this paper contains some miscellaneous remarks on the geology of the island of Rona. The principal rocks that here make their appearance are gneiss and hornblende rock (including under the latter denomination both hornblende slate and green-stone slate). Where these two rocks come in contact, the gneiss is irregularly curved and contorted. The gneiss is traversed by numerous and thick veins of graphic granite in which wolfram occurs.

The district of Assynt, forming the western part of Sutherlandshire, is the subject of the next article. The mountains and higher ground of this district consist of the same rock as the so called granular quartz of Jura, forming here, as in the last-mentioned island, smooth conical hills of considerable elevation, snow white at their summits, and

singularly steril and arid. The white colour of the rock is however only superficial, the recent fracture exhibiting gray, yellow, and brown tints. It is distinctly stratified, and rises at a high angle. The texture of this rock is various, from imperfectly conchoidal to loosely granular, composed of rounded grains, and in some beds of angular fragments. It divides naturally into rectangular blocks, on the surface of which is the appearance as if of cylindrical bodies imbedded in the mass, forming a number of circular protuberant spots, of a white colour and more compact texture than the rest of the rock. A section at right angles to the natural surface of these blocks, shows that the above-mentioned circular spots are occasioned by the cross fracture of straight cylindrical bodies, which are perhaps the remains of some species of sabella. Associated with this grit are compact gneiss, hornblende slate, and syenitic granite, but their relative positions Dr. M. was unable to ascertain. Subordinate to and apparently alternating with this grit is a great deposit of limestone in two very thick stratified beds, with a thick kind of grit interposed: in some parts the section of these beds forms a continuous and even line, but in other parts is so curved and broken that the stratification can scarcely be perceived.

The limestone is a dark gray or nearly black, of an earthy aspect and minute granular fracture, and smelling offensively when rubbed. It does not appear to contain organic remains, but is traversed by veins of red or white calcareous spar. It contains grains of sand, and therefore gives fire with steel. Its surface is covered for the most part with a loose calcareous tufa, which in some places being rendered solid by an infiltration of calcareous matter constitutes a hard breccia.

In the same valley of the Tain, of which the above rock forms the precipitous side, occur insulated masses rising through the gross of unstratified granular marble, varying in colour from pure white to gray, the geological relation of which Dr. M. has not been able to determine.

This is the white marble mentioned by Williams in his "Mineral Kingdom," and which has since been wrought with some success by Mr. Toplin, of Gateshead.

May 21, 1813.—The President in the chair.

William Hill, Esq. of Bedford row,
Hastings Elwin, Esq. of Farnham, Dorset,
Frederick Daniell, Esq. of Lincoln's Inn Fields,
were elected Members of the Society.

A paper by the Rev. William Gregor, Hon. M.G.S. containing

containing "Observations on a Species of Tremolite found in Cornwall," was read.

This mineral occurs in a dark green serpentine rock, forming the ridge called Clicker tor, in the neighbourhood of Liskeard. It is accompanied by asbest. On analysis it appears to be composed of

62·2	silica.
14·1	lime.
12·9	magnesia.
5·9	oxide of iron.
1·0	water.
	a trace of oxide of manganese and of soda.
96·1	
3·9	loss.
100·0	

The continuation of Dr. MacCulloch's paper on the Geology of different parts of Scotland was read, and thanks were voted for the same.

The granular quartz of Isla appears to be precisely the same rock as the sandstone of Jura already described. From the observations of Prof. Jameson, coinciding with those of Dr. MacCulloch, it appears to alternate with mica slate and clay slate, and with a very important formation of limestone. This limestone is more or less granular, and contains no organic remains, nor any beds of fetid limestone: when inclosed between beds of clay slate, it is of a dark blue colour; when in contact with mica slate, it is gray or white: both varieties pass insensibly into the slate within which they are inclosed; and the limestone, the schistus, and the sandstone, are evidently members of one formation.

The structure of Schehallien is the subject of the next article. This mountain consists of a central ridge in vertical strata, flanked on every side by beds of mica slate nearly vertical, and containing subordinate beds of limestone. The rock composing this central ridge, though it has been denominated granite by some mineralogists of no mean name, is in fact the same as the granular quartz of Jura, being composed of highly compacted grains of quartz, with interspersed grains of earthy felspar. The same quartz rock appears in the valley of the Lyon, to the south of Schehallien, and it seems that the mica slate alternates with beds of quartz rock, and is therefore of the same æra as this latter.

The vicinity of Crenian, which is the subject of the next article, is remarkable for presenting nearly vertical beds of

 well

well characterized grauwacke and grauwacke slate, with equally well characterized beds of clay slate and chlorite slate.

The structure of the rocks bounding the vale of Aberfayle is next described. On tracing this country up to Ben Ledi, alternations of grauwacke and grauwacke slate with clay slate first occur: then comes a fine roofing slate approaching in parts to mica slate, but distinguished by a true grauwacke structure, that is, of grains united by a slaty cement: only in this case the cement is not clay slate, but mica slate: beyond this the true mica slate makes its appearance.

The general deduction from these facts is, that those rocks which have been ranked as primitive schist alternate with rocks of recomposed materials, which belong to the transition class of Werner: but this alternation throws great doubt on the reality of transition rocks, as distinguished from primitive, and rather tends to bring back the original division of rocks into primitive and secondary.

PHILOSOPHICAL SOCIETY OF LONDON.

The attention of the Society has lately been directed to two Lectures on Pneumatic Chemistry delivered by the Registrar, Mr. Miers.

After some prefatory observations intended as an introduction to the study of the science, he proceeded to give an abstract of the various theories of chemical affinity, commenting particularly on the beautiful system of relative proportions of Mr. Dalton, and the grand yet simple doctrine of electrical energies of Sir H. Davy,—doctrines which lead us fairly to indulge in the hope of our being on the eve of an important period, when chemical laws shall be submitted to calculation, and the whole science elucidated by mathematical principles. All kinds of matter existing in the universe that are cognizable to our senses, and that may be denominated elementary, of which there are forty-six, he divided into two classes—Combustible, and Supporters of Combustion. The individuals of the latter class, comprising only oxygen and chlorine, are distinguished principally by ranging themselves round the positive pole in the Voltaic circuit; the former class comprehending all the remaining forty-four undecompounded bodies, which are distinguished as ranging themselves round the negative pole in the Voltaic circuit, and as opposing themselves in the relations of affinity to the bodies of the other class. He then entered on the consideration of the known properties

properties of oxygen, chlorine, and their curious combination euchlorine. Hydrogen next followed, its combination with oxygen—water,—and with chlorine, muriatic acid gas. With respect to the nature of chlorine and muriatic acid gas, Mr. M. entirely coincided with the views of Sir H. Davy on this subject. Independent of the principal classic property of chlorine, that of its being like oxygen an indecomposable body ranging itself round the positive pole in all Voltaic circuits, facts have arisen from discussions in the scientific journals, which have satisfied most chemists as to the simplicity of its nature. What has tended most to the confirmation of this opinion is the discovery of phosgene gas, (a combination of chlorine and carbonic oxide gases,) by Mr. John Davy. According to the French system, the union of these two gases should have produced carbonic acid and muriatic acid gases; but what ultimately decides the correctness of Sir Humphry Davy's notion is the formation of this new gas, which possesses characters so peculiar. Agreeing entirely with Sir H. Davy in the nature of chlorine, he, however, differed with him in the nomenclature proposed by him for the designation of its combinations. Sir H. D. proposes to distinguish the salts formerly called muriates, which are according to him combinations of chlorine with metals, by the terminating syllables ane, anea, anie: independent of the probability of mistake in expressing the last syllables, there is in all cases an objection to making distinctions of species in verbal terminations; and as the combinations with oxygen are called oxides, there is no sufficient reason why those with chlorine should not be called chlorides; and as distinctions of the proportions of combinations, there may be given—prochlorides, deuchlorides, trichlorides, and answering to those proposed by Dr. Thomson, of protoxide, deutoxide, &c.* There may, perhaps, at some early period, be proposed a better method than either of these; at present both are liable to objections.

The second lecture commenced with a consideration of nitrogen, when he successively went through its combinations with oxygen, nitrous oxide gas, nitrous gas, nitrous acid gas, nitrous acid, nitric acid, and atmospheric air. It has been a subject of much speculation with natural philosophers, whether the two components, nitrogen and oxygen, existed in a state of chemical combination in atmospheric air, or merely in a state of mechanical mixture.

* The only objection to this mode of distinction is the length of the term.

 From

From its possessing no other properties than those of the individual gases, and from artificial mixtures in various proportions always comporting themselves in similar relations, it has been universally agreed to be merely a mechanical mixture of the two gases. But, as the composition of the atmosphere is uniformly the same in whatever situation it is found, this opinion seemed to the lecturer somewhat problematical. As so vast a quantity of oxygen is constantly consumed in respiration for the support of animated creation, and as the proportion of the oxygen to the nitrogen is ever invariably the same, it has never been satisfactorily explained how the equilibrium is restored. There most probably exist some unknown processes in nature, by which a quantity of oxygen is generated corresponding to that consumed by animals, by the combustion of inflammable bodies, &c. To this it has been said, that the vegetable world is the organ by which this supply is effected; that the leaves of plants absorb the carbonic acid formed in respiration, which retain the carbon and give out in return the oxygen. To this however the lecturer stated many objections. How is it that in the depth of winter, when a complete check is given to vegetation, when plants possess no leaves to effect a supply, when at the same time the consumption of oxygen is so considerably increased by the combustion of inflammables for the generation of warmth, &c. how is the oxygen in such times restored? This objection it does not appear easy to obviate. There most probably exist some provisions of nature of which we are entirely ignorant, and these perhaps are only to be found in the nature of nitrogen.

With respect to the nature of nitrogen he entered at some considerable length, and gave a minute history of the earlier experiments of Priestley, of Göttling, Wiegleb, and Crell; of Dieman, Van Stroostwyck, of Van Hausch Juch, Van Mons, of Girtanner, and of Berthollet and Lagrange.

The German chemists had found water converted into nitrogen when passed in a state of vapour through ignited earthen tubes, and hence concluded nitrogen to be a compound of water and caloric. The Dutch chemists denied this conversion of water into nitrogen, conceiving its presence to have been derived from the introduction of atmospheric air through the interstices of the tube. Girtanner, on the contrary, asserted the correctness of the German chemists, and confirmed by experiments of his own the formation of nitrogen, though he differed with them as to its nature. He conceived nitrogen to be a combination of hydrogen

drogen and oxygen, or water deprived of part of its oxygen. He found nitrogen to be produced in all cases where water in a state of vapour was passed over any matter at a high degree of heat, that would abstract part of its oxygen. He denied that this change was owing to the introduction of atmospheric air through the pores of the tubes, for he found the same when they were covered externally with a glass, or with a glass tube placed in an earthen or metallic one, provided any metallic body were placed within. Berthollet and La Grange, however, were induced from their importance to repeat the experiments of Girtanner, but with all their attention to accuracy could not detect the smallest portion of nitrogen. This positive contradiction of two chemists so justly famed for the accuracy of their experiments, brought the assertions of Girtanner into disrepute, who, from the looseness of his style, was perhaps justly considered as an inaccurate chemist and an enthusiastic theorist. This opinion of chemists concerning the nature of nitrogen was most perfectly settled: since this time it continued to be considered as a simple body, till the brilliant discoveries of Sir Humphry Davy burst forth on the science of chemistry with a most dazzling splendour, and diffused a new light on the nature of the material world. Sir Humphry Davy, induced by his discoveries of the nature of the fixed alkalies and earths, was led to examine the volatile alkali ammonia, and, from some experiments detailed in his second Bakerian lecture, concluded that oxygen entered into its composition. This he could only account for by supposing either hydrogen to be an oxide of nitrogen, or nitrogen to be an oxide of hydrogen. About the same time Messrs. Allen and Pepys were engaged in experiments on the products of respiration, who found that under certain circumstances there was a loss of oxygen and a considerable production of nitrogen. Sir H. Davy, continuing his experiments, found a considerable loss of nitrogen during the action of the fusible compound of potassium and nitrogen on matter containing oxygen, its place being supplied by a corresponding production of hydrogen and oxygen; he hence concluded that "the decomposition and composition of nitrogen seem proved, allowing the correctness of the data, and one of its elements appears to be oxygen." Sir H. Davy, however, from his more recent researches and more refined experiments, began to doubt the accuracy of his previous conclusions; he has now in a great measure renounced his former views of the nature of nitrogen, and now again classes it among the simple substances.

Towards

Towards the beginning of the last year, the attention of the lecturer was drawn to this subject by some experiments he was then performing, where he found nitrogen produced from water when he was certain it could not have been derived from the atmosphere. From the results of these experiments he was led to consider nitrogen as a combination of hydrogen and oxygen, or of water with hydrogen. As results of such importance require to be confirmed by the most decisive and unequivocal proofs, and as experiments of so much magnitude, involving in them such weight of consequences to the present theories of chemistry, demanded, before they were ushered into public notice, the most clear and decided results, they were unavoidably reserved for more refined experiments, which from circumstances had been deferred to the present time. He mentioned his intention of immediately resuming his labours, which, when sufficiently mature, he should lay before the Society. There are many analogies he pointed out that lead us to consider it a compound body; but did we possess no other knowledge of this substance than such evidences, we might be led to consider it a simple body. The principal objection now urged against such a conclusion is, that nitrogen is not affected when ignited in the most intense Voltaic circuits, and that, when even exposed to a similar action with potassium and other inflammable substances, no change is perceptible. These objections he conceived ought not to have any very considerable weight, as we know that the curious compound of phosphorus, chlorine, and ammonia, which is so easy of formation, cannot be again separated into its original elements by any methods of analysis; and that, on the contrary, nitrogen and chlorine which have been exposed to the action of the most violent Voltaic arrangements with a view to combine them, but without success, have been recently made to enter into combination by a process as simple as can well be imagined. The evidence, then, of the inaction of the Voltaic influence is not fatal to the idea of its compound nature.

He next proceeded to consider the combinations of nitrogen with chlorine, forming the curious detonating compound lately discovered; of nitrogen with hydrogen, forming the volatile alkali ammonia; and of its metallizing basis ammonium.

Phosphorus next in order now came under consideration, when he continued to treat on its three combinations with oxygen and with chlorine, with hydrogen and with other bodies, forming compounds well known to every chemist.

THE

THE ROYAL MEDICAL SOCIETY OF EDINBURGH

Propose as the subject of the Prize Essay for the year 1815, the following question:

"Is azotic gas absorbed in the lungs during respiration? —If it is not, whence do herbivorous animals derive their azote?"

A set of books, or a medal of five guineas value, will be given to the author of the best dissertation on an experimental investigation of the subject proposed by the Society; for which all the members, honorary, extraordinary, and ordinary, are alone invited as candidates.

The dissertations are to be written in English, French, or Latin, and to be delivered to the Secretary on or before the first of December of the succeeding year to that in which the subjects are proposed; and the adjudication of the prize shall take place in the last week of February following.

To each dissertation shall be prefixed a motto, written on the outside of a sealed packet, containing the name and address of the author. No dissertation will be received with the author's name affixed; and all dissertations, except the successful one, will be returned, if desired, with the sealed packet unopened.

KIRWANIAN SOCIETY OF DUBLIN.

May 19th.—A paper "On the question whether alcohol be a product of fermentation or of distillation," was read by M. Donovan, Esq. Secretary.

The paper commenced with a sketch of the ancient opinions on the question, and of the evidences upon which they were founded. It was then stated, that perhaps the first chemist who doubted the existence of alcohol in fermented liquors was Rouelle the elder, who was led to this supposition from reflecting on the fact that spirit is not produced until the wine or other liquor begins to boil. Nearly the same opinions, it was observed, were afterwards maintained and enlarged upon by Fabroni. The Florentine philosopher found that by means of sub-carbonate of potash he could detect $\frac{1}{100}$th part of alcohol purposely added to new wine; while, when pure wine was employed, the same test produced no appearance whatever: from this and some other experiments he concluded "that alcohol is a product of distillation, and not of fermentation." After noticing the remarks and opinions of several other chemists, it was observed, that no experimental objections of any importance have been offered to Fabroni's assertions until

until lately, when a most ingenious paper by Mr. Brande, appeared in the Philosophical Transactions (1811). Upon the reasonings and experiments of that excellent chemist several observations were made, all of which terminated in the following conclusion, "that his refutation of the grounds of Fabroni's objections to the common opinion, was decisive and complete;" but it was observed that still a few experiments tending to prove that the common opinion is correct, might not be considered superfluous.

The author then remarked, if the temperature specified by Fabroni as adequate to the decomposition of wine be really so low as 63, that the legitimate consequence would be directly in contradiction to the opinion which the Florentine philosopher laboured to establish: for in almost all cases of fermentation in the large way, the temperature rises to, and most frequently much above 63.

Several experiments were then detailed which tended to prove that the alcohol is produced in the process of fermentation. A fermentation was conducted which never rose beyond 57: by peculiar management, the vapour of alcohol was extricated from this wash, which was capable of catching flame from an ignited body. From another portion of the same wash, the alcohol was separated in the insulated form perfectly pure, very strong, and highly inflammable. Yet in all these experiments the temperature never rose to 60, which is $3\frac{1}{2}$ degrees below that stated by Fabroni as necessary to the formation of alcohol.

After detailing the remainder of his experiments, the author stated, that from all these he thought he was warranted in concluding "that alcohol is a product of fermentation, that it exists ready formed and perfect in fermented liquors, and that it exists in them in a state of very loose combination with water and vegetable matter."

Mr. Donovan repeated the principal part of his experiments before the Society.

IMPERIAL INSTITUTE OF FRANCE FOR THE YEAR 1812, DRAWN UP BY M. CUVIER.

[Continued from p. 316.]

Botany and Vegetable Physics.

Most physiologists have long admitted that there is in plants an ascending sap, which proceeds from the roots to the branches, and contributes to the development of the branches in length; and a descending sap which descends from the leaves to the roots, and to which some ascribe the chief agency in the development of the wood, and consequently in the swelling of the trunk. M. Fe-

M. Feburier, a nurseryman at Versailles, has endeavoured to collect these two kinds of sap separately: with this view he made a deep cut in the trunk of a tree, and filled a bladder to the lower aperture, so that nothing should enter but the liquid coming from the parts of the tree situated below: he then made another incision, and placed the bladder at the upper part of it so as to receive nothing but the sap coming from above.

M. Feburier regards the sap collected in the lower bladder as ascending, and the other as descending juice, and gives numerous observations on the proportions of both under various circumstances. Wishing afterwards to be certain as to the route which each sap takes in the interior of the vegetable, he plunged alternately by the two extremities branches of trees into coloured tinctures. In both cases, these tinctures appeared to him to follow the ligneous fibres of the medullary canal, which made him ascribe the same progress to the two saps, in which he is at variance with the result of other experiments made by M. Mustul.

M. Feburier is also of opinion that the ascending sap contributes chiefly to the development of the branches: the descending sap to that of the roots; but he thinks that the cambium, or that humour which transudes horizontally from the trunk, and which has been regarded as the matter which gives to the tree its growth in thickness, results, as well as the peculiar juices, from the mixture of the two saps.

The presence of the leaves necessary for producing the descending sap is also of consequence for the increase in thickness; but the buds, which M. du Petit Thouars makes to play a great part in this operation, have really no share in it according to M. Feburier; for it takes place, he informs us, while the leaves exist, and it ceases immediately when they are removed, whether buds are left or not.

So far as regards the flowers and fruits, M. Feburier says he has observed that the ascending sap, when it predominates, tends to determine the production of the simple flowers and the complete development of the germs; that the descending sap, on the contrary, where it is superabundant, produces the multiplication of the flowers and the petals, and the enlargement of the pericarps, and consequently of the pulpy part of the fruit: principles from which it will be easy to draw many useful hints, and which will also explain several practices already adopted.

According to M. Feburier, the soft part of the wood when laid bare, but protected from the contact of the air, is capable

capable of reproducing, by means of the cambium, the liber and the bark necessary for covering it, as the bark produces habitually, and even when it is partly removed from its trunk, liber and soft wood. In this point he has for his antagonist our colleague M. Palisot de Beauvois, who has also directed his attention to these difficult questions respecting the progress of the sap and the formation of the wood. According to this botanist, this oozing out of a glairy matter, which some physiologists suppose to flow from the old wood, and which contributes to the formation of the liber, is not founded upon real experiments. On the contrary, when part of the bark of a tree has been removed, and the wound well rubbed, so as to leave no liber nor cambium, neither the soft wood nor the wood itself produces any thing, but the lips of the solution of continuity made in the bark stretch out, cover the wood left bare, and then produce liber and soft wood incontestably emanating from this bark. M. de Beauvois announces that he will soon explain this proposition fully, which he has merely hinted at in a memoir on the marrow of vegetables.

The opinion of physiologists has been hitherto much divided, as to the utility and functions of the pith of vegetables. According to some, this organ is necessary to the life of the plants during their whole existence: according to others, it is useful to them only during the first year and only during the whole of the time that it is green and succulent, and when it may be still easily confounded with the cellular texture.

M. de Beauvois has made upon this subject some observations which tend to show that the marrow exercises, during the whole life of the plants, functions, if not of an absolute necessity to their existence, at least very important to their progress, and the development of their branches, leaves, and particularly the organs necessary for their reproduction.

He has remarked that the medullary canal, *i. e.* the circular layer of fibres which immediately surround the mass of the pith, has always a form corresponding to the arrangement and the disposition of the branches, boughs, and leaves; that in the vegetables with vertical furrowed (*verticillées*) boughs and leaves, for instance, the horizontal section of the medullary tube shows as many angles as there are boughs at each stage and at each *verticille*.

Thus the medullary canal of the red laurel presents an equilateral triangle if the branch below the *verticilles* has three boughs and three leaves; but if we cut it below the lowest

lowest verticille, from which a leaf and a bough frequently fall off, there will be two angles only, and the vestige of a third equally abortive. This law is constant, even in the herbaceous plants.

M. de Beauvois has begun similar observations on the plants with opposite leaves, those alternating, distic, repeated spiral, and composed of four, five, and a greater number of boughs and leaves. He thinks it probable that there are the same relations between the form of the medullary canal and the disposition of the branches, the boughs, and the leaves. For example, the opposite leaves seem to necessitate a round medullary canal, and which becomes oval, having the extremities more and more acute the nearer it approaches the point of insertion of the boughs and leaves.

When the leaves are alternate, the circle is less perfect, the extremities are thinned off equally, but alternately, and each on the side on which the bough ought to appear.

When the leaves are spiral, the number of the angles of the medullary canal is equal to that of the leaves of which the spirals are composed. It is thus that the medullary canal of the linden tree has only four angles; that of the oak, the chesnut tree, the pear, and almost all fruit-trees, &c. has five angles more or less regular, because the spirals are multiplied, and succeed constantly by fives.

Grew and Bonnet seem to have been the first to make these observations. The former had observed very singular forms in the medullary canal, particularly in that of the pivoting roots of pot-herbs; but he has not seized the relations of these forms with the dispositions of the boughs and the leaves. The latter directed his attention to distinguish the vegetables with opposite leaves verticillated, alternated, spiral, but has not made a comparison of these dispositions with the form of the medullary canal.

M. de Mirbel has continued his researches in the structure of the organs of fructification in vegetables, in which he has been most zealously seconded by M. Schubert, who was sent to France by the Government of Warsaw to acquire the science of botany preparatory to his publicly teaching it in Poland.

These two botanists have examined all the genera of the family of the prickly trees, or the coniferæ; trees of the first importance, on account of the singularity of their organization, the magnitude of the species, and the utility of their products. Every person can distinguish at the first glance the cedar, the pine, the yew, the juniper, &c.; but although botanists have studied with particular attention the

the organs of reproduction in these vegetables, they are not agreed as to the characters of their female flower; or rather, most of them agree that the stigma of the pine, the fir-tree, the cedar, and the larch-tree, is still to be found. We may therefore say that these trees are in this respect species of cryptogama. Messrs. Mirbel and Schubert go still further: they assert that the female flower of the yew, the juniper, the cypress, &c. is no better known, and that, without exception, all the genera of the family of the coniferæ have a common character, which has hitherto deceived observers, and which consists in the existence of a cupule, not like that of the flower of the oak, which covers the basis only of the ovary, but much more hollow, concealing entirely the ovary, and closed like a spout at its orifice. The female flower contained in this envelope has escaped observation. In the arbor vitæ, the yew, the juniper, the cypress, &c. the cupule is folded back, and by an error accounted for by the extreme smallness of the organs, from time immemorial the orifice of this cupulus has been taken for the stigma. In the cedar, the larch, the pine, and fir-trees, the cupule is reversed, and the orifice is scarcely discernible. It is only of late years that it has been observed in England by Mr. Salisbury, and in France by Messrs. Poiteau, Mirbel, and Schubert. These botanists have not hesitated to consider it as the stigma; which was natural, since it has been agreed to place the stigma of the yew, the arbor vitæ, the cypress, &c. at the orifice of the cupule. But ulterior researches have undeceived Messrs. Mirbel and Schubert. By means of a delicate anatomical inquiry, they have ascertained that what is generally taken for the female flower in the coniferæ is nothing else than the cupule; the form of which closely imitates that of a pistil, and which contains in its cavity the true flower, which is provided with a membranous calyx adhering to the ovary, and with a stigma sessile in all the genera except in the ephedra.

It may be easily conceived that this structure, so different from what had been hitherto imagined, brings with it great changes in the description of the characters of the family and of the genera.

According to M. Mirbel, the female flower of the plants of the family of the cycas has an organization analogous to that of the coniferæ; which supports the opinion of M. Richard, who places these two families beside each other among the dicotyledons: but M. Mirbel thinks, that while the characters of vegetation will serve as a basis to the two great

great divisions of vegetables with visible flowers, the cycadeæ could not be far removed from the palm trees.

The organization of the male flower of the mosses has been also the subject of the researches of Messrs. Mirbel and Schubert. After Hedwig, it would have been difficult to discover any new facts on this subject. But the rupture of the antheræ and the emission of the pollen were phænomena which several botanists call in question. Our two botanists assert that they are presented in the most unequivocal manner to our eyes. The organs which Hedwig calls males in the *polytrichum commune*, placed upon water, are cleft into a beak at their summit, and sent out an oleaginous liquor, which extended like a slight cloud over the surface of the liquid. Messrs. Mirbel and Schubert then submitted comparatively to observation the pollen of a great number of phanerogamous plants, and they saw that it acted in the same way as the male parts of the mosses: which leads them to believe that those parts designated under the name of antheræ by Hedwig, might possibly be nothing else than simple grains of naked pollen of a particular form.

M. Mirbel by himself has continued his inquiries into germination. He remarks, in spite of the opinion generally received, that the radicle does not always first come out. For instance, in many cyperaceæ it is constantly the plumule which first appears.

The same botantist has thrown new light, with important modifications and additions, upon his opinions respecting the organization of the stalk, their development, and the structure both internal and external of the organs of fecundity in plants.

M. Henry de Cassini, the son of one of our members whose name is so well known as an astronomer, has presented to the Class a memoir which augurs happily for his future success in another science. He has examined with particular care the style and the stigmata throughout a whole family of plants well known by the names of composites, syngenesia, or synanthereæ, and organs so inconsiderable presented to him a crowd of curious variations, which appeared to him strong enough to lay the foundation of a division of these plants founded solely upon the modifications of those two parts of the pistil.

We regret that we are unable to follow this accurate observer through all the details upon which he has entered, and which he has described and drawn with singular precision: it cannot be doubted that they will one day serve to perfect the classification of this family, which is so nu-

merous and so natural, and the subdivision of which ought consequently to be more difficult than any other.

There are few families of vegetables so directly useful to man as that of the grasses, among which we reckon oats, barley, wheat, rice, maize, the sugar cane, hay, &c. &c. The bare mention of the name of these plants is sufficient to show the importance of any work which shall describe them with accuracy.

The characters hitherto have been generally regarded as insufficient. At every step the observer is arrested: and it is difficult, nay, often impossible to discover the true genus of the plant under examination: frequently also, the characters adopted agree with certain species only, and are no longer recovered in the rest of the genus.

M. Palisot de Beauvois has undertaken on the subjects of this family a general work which he has entitled, An Essay upon Agrostography. He has endeavoured to remove all confusion, and to give to each genus signs which are constant and easily discovered, so that no observer can be mistaken.

With this view he has been obliged to adopt new bases, which he has already announced in his Flora of Oware and Benin, and which chiefly belong to the separation or junction of the sexes to the composition of the flower and the number of its envelopes.

Twenty-five plates, in which all these characters are represented, facilitate the study of these plants which interest all classes of society, and even those who do not study botany as a science.

M. Beauvois continues his Flora of Oware and Benin, the thirteenth number of which is published; and his History of Insects, collected in Africa and America, the eighth number of which has appeared.

M. de la Billardiere has continued and finished the collection of his rare plants of Syria and Liban, by the publication of the fourth and fifth numbers.

The same naturalist has communicated to the Class several interesting observations in natural history which he made in his voyage to the Levant, the publication of which was interrupted by the longer and more dangerous voyage which he since undertook along with d'Entrecasteaux, and the account of which has been before the public these many years.

M. Gouan, corresponding member of the Class at Montpelier, has published a description of the generic characters of the ginko-biloba, a singular tree of Japan, which had

had long existed in Europe, but which, from its not having flowered, could not having been arranged in the system of vegetables.

The leguminous plants are not less important, as furnishing a number of nutritious articles of food for men and animals, various pharmaceutical substances, several gums employed in the arts, and some precious woods: but, like all the very natural families, it is difficult to subdivide them with precision; which is nevertheless the more important, as the number of the vegetables which it contains is already very considerable, and more are discovered every day.

M. Jaume Saint Hilaire has presented to the Class a memoir accompanied by several drawings made by himself, in which he claims new characters for the leguminous plants, founded chiefly upon the form of the fruit, and which appear to this botanist more constant and more easy to seize than those formerly employed. He adds besides several new genera to those which are already admitted.

There is a family much less important in its uses, but much more singular in its characters, and which is only to be found on the sea-shore: this is the family of the fuci and analogous marine plants. M. Lamouroux, professor of natural history at Caen, has made them one of the chief objects of his study. He gives them the common name of *Thalassio-phytes*, and divides them into several tribes, the characters of which he was obliged to take from all the parts of the vegetable, for want of finding a sufficiency in the organs of fructification, which generally serve as a basis to these kinds of distributions, but which are too little known in most of the fuci to enable us to have recourse to them solely.

We regret that we cannot enter more fully into this valuable paper, and have to add our wishes for its speedy publication.

Zoology, Anatomy, and Animal Physiology.

M. Geoffroy Saint Hilaire, who has repeatedly directed his researches to the natural history of the bat, and has made us acquainted with so many interesting species, purposes to give a general view of all the species. As a prelude to this work, he has written a dissertation upon the rank which these singular animals ought to occupy among the mammiferæ. They have been long regarded as an intermediate genus between quadrupeds and birds, and it is at least equally true that they hold a kind of middle place between quadrumanous and carnivorous animals. In short,

amid the multitude of arrangements proposed by naturalists, they are alternately (according to the last edition of Linnæus and Brisson) allied to the *quadrumani,* and (according to the plan and the former edition of Linnæus) they are placed among the lesser carnivori or insecto carnivori, like the mole and hedge-hog. Some naturalists, among whom are Messrs. Storr and Cuvier, place them at the head of the carnivori, and before those insecto-carnivori just mentioned, and immediately after the *quadrumani*; with this difference, however, that M. Cuvier distinguishes them more particularly, and as a subdivision. Others also, such as Ray and Blumenbach, Lacepede and Iliger made a separate order of them, and this order is placed by Ray and Lacepede in some measure out of its proper place: by M. Blumenbach between the quadrumani and the other animals with long nails, at the head of which this naturalist places the gnawing animals: lastly, by M. Illiger after the *edentes* and before the carnivori, at the head of which stand, as in the arrangement of M. Cuvier, the insecto-carnivori.

It may be easily conceived that all combinations must necessarily depend upon the organs to which each naturalist may have paid most attention. Those who have paid most attention to the skeleton, to the intestines, to the organization of the feet, to the form of the nails, and to the grinders, have allied the bat to the carnivori, and this appears to be the most generally received opinion: those who have particularly noticed the *incisores,* the position of the mammæ, and the pendulous penis, have allied this animal to the quadrumani.

M. Geoffroy, in the work above alluded to, insists most upon these last resemblances, which he thinks have not been sufficiently attended to; but he shows particularly that the singular prolongation of the anterior extremities, the general tendency of the skin to stretch excessively, and the peculiar properties which bats thence enjoy both with respect to their sensations and their motions, require that a separate order should be formed of these mammiferæ, at the same time that their various resemblances with the quadrumani and the carnivori show that they ought to be placed between them. We may look with much anxiety for the subdivision of this order, as well as the detailed history of the species which M Geoffroy has promised us.

M. de Lamarck, who is intrusted with teaching at the Museum of Natural History every thing connected with animals without vertebræ, published some years ago the work which serves as the basis of his lectures, in which he explains

explains in a way peculiar to himself the classes, orders and genera of these innumerable animals: but as travellers have since discovered many new species and genera; as anatomists have better developed their structure; and lastly, as the discrimination of M. de Lamarck has discovered several new relations between them, he has published an abridged syllabus of his course according to this perfected method, in which he contents himself with indicating the characters of the superior divisions, and merely gives the simple nominative enumeration of the genera.

He follows in point of arrangement, the order of the degrees of complication, commencing with the most simple animals. Supposing that those which have no nerves apparent, are moved only in virtue of their irritability, he denominates them *apathic animals:* he gives the name of *sensible animals* to others without vertebræ, and reserves that of *intelligent animals* for those with vertebræ. To his old classes, which are already well known to naturalists, he adds that of *cirrhipedes*, which comprehends the *sea glands*, and their analogous genera, and which he places between these *anelides* and *mollusci*; that of *epizoary* or intestinal worms, which he places among his apathic animals; and that of the *infusores*, or microscopic animals without mouths or apparent intestines. He leaves the echino-dermes among the *radiarii* and the apathic animals, and in a greater degree of simplicity than that in which he places the intestinal worms.

We regret that want of room does not admit of our making known the other changes introduced by M. de Lamarck in his orders, nor the numerous additions which he has made to the list of genera; but naturalists will not fail to examine them in the work itself.

Notwithstanding the success of the anatomical researches respecting animals without vertebræ for these few years past, there still remained one of their families, the fundamental organs of which were not yet well known: these are the echino-dermes, which comprehends the sea stars and analogous genera. The Class having proposed a prize for the improvements of this branch of comparative anatomy, it was gained by Professor Tiedman of the University of Landshut. The memoir of this eminent anatomist makes known for the first time, with rare precision, many particularities of organization peculiar to these singular animals. A kind of circulation is easily perceived between their organs of digestion and of respiration, without presenting however a complete double circle: besides, the

 branches

branches cannot be followed into the external organs, nor into those of motion: it even seems, according to M. Tiedman, that a vascular system totally different is distributed to the numerous pedunculi, which in these animals serve as instruments of locomotion.

The organs of respiration differ materially according to the genera; in the holothurii they represent hollow trees, the vessels of which are filled or evacuated from the external water, and are interlaced with a vascular net-work. In the sea stars and bears, the water penetrates immediately into the cavity of the body and visits every part of it.

This elegant work was accompanied by some very fine drawings executed by M. Martin Münz, doctor in physic, and appeared to the Class well deserving of the prize, from the quantity of new facts and observations which it contains, and from the addition which it must make to our knowledge of the echino-dermes, although the problem proposed as to their circulation has not been resolved in a manner completely satisfactory.

A family much simpler in its organization than that of the echino-dermes, but much more numerous in species, that of the corals and other animals composed of a solid case, has been specifically arranged by M. Lamouroux. This naturalist has made an extensive collection of those whose base is not stony, and which present forms often singular and agreeable: by comparing with great care the form, the mutual position of the cellules from which the polypi issue, and all the other different appearances, he purposes adding twenty-eight new genera. This is a work of unquestionably great utility to the improvement of our knowledge of the animal kingdom, but from its nature it does not admit of an abridged analysis. A speedy publication of the entire memoir will be highly gratifying.

M. Cuvier, purposing soon to commence the printing of his great work on Comparative Anatomy, which has occupied his attention for so many years, has presented to the Class the table of the divisions according to which the animal kingdom ought to be distributed in this work. For a long time naturalists were struck with the great differences which distinguish the invertebral animals from each other, while the vertebral animals resemble each other in so many respects. Hence resulted a great difficulty in drawing up their comparative anatomy; the animals with vertebræ being easily generalized, but not the others: a remedy however has been suggested for this difficulty: from the way in which the propositions relative to each organ were always

always grouped, M. Cuvier concluded that there exist among animals four principal forms, the first of which is that with which we are acquainted under the name of vertebral animals, and of which the other three are nearly comparable to it by the uniformity of their respective plans. The author denominates them *mollusci*, articulated animals, and radiated animals or zoophytes, and subdivides each of these forms or ramifications into four classes, according to motives nearly equivalent to those upon which the four classes rest which are generally adopted among the vertebral animals. He has derived from this in some measure symmetrical arrangement, a great facility in reducing under general rules the diversities of organization.

The comparison which the same member has drawn of the osteology of vertebral animals, has furnished him with some new ideas as to the osseous structure of the head in this branch, and which he has also presented to the Class.

It had been long since ascertained that oviparous vertebral animals, *i. e.* birds, reptiles, and fishes, had several common relations of organization, which made them differ from the viviparous or mammiferous vertebral animals; M. Geoffroy Saint Hilaire had even presented some years ago an extensive and elegant work, of which we gave an account at the time, in which he proved among other things the identity of structure of the heads of all the ovipari, and the relations of the numerous pieces which enter into their composition, with those which we distinguish in the fœtus of the mammiferæ, in which, as is well known, the bones are much more subdivided than in adults.

M. Cuvier, adopting the views of M. Geoffroy, has endeavoured to determine in a certain manner, to what bone of the head of the mammiferæ each groupe of bones of the head of the different ovipari answers; and he thinks he has attained this, by adding to the analogy of the fœtus of the former, the consideration of the position and of the functions of the bones: *i. e.* by examining what organs they protect, to what nerves and vessels they give a passage, and what muscles are attached to them.

M. Jacobson, surgeon major in the armies of the king of Denmark, has made the Class acquainted with an organ which he discovered in the nostrils of quadrupeds, and with which no anatomist seems to have been acquainted. It consists of a narrow sac, lying along the cavity of the nostrils, defended by a cartilaginous production, covered internally by a mucous membrane, doubled in part by a glandulous texture, receiving some

very remarkable nerves which are very distinct divisions of the first pair, and opening chiefly into the palate, behind the incisores, by a channel which passes through the hole denominated incisive by anatomists. This organ does not exist in man, and is more distinct in most of the herbivorous than of the carnivorous animals. It must be presumed that it is connected with some of the faculties which nature has granted to quadrupeds, and refused to our species; such as the faculty of rejecting venomous substances, or of distinguishing the sex and state of heat, &c.

The particular history of animals has been enriched with many important works and interesting observations.

M. de Humboldt has published the first volume of his Observations on the Animals of America, in which he enters not only upon different inquiries as to the condor, the electrical eel, the crocodile, and many other subjects which we stated in our preceding analysis; but he has also given several entirely new memoirs, particularly one upon the apes of the new world, eleven or twelve species of which only had been described by Buffon and Gmelin, but which M. de Humboldt, by adding his own observations to those of M. d'Azzara and Geoffroy Saint Hilaire, extends to forty-six.

He has recently read to the Class another memoir intended for his second volume, and in which he describes two new species of rattlesnakes which he discovered in Guyana.

The tempests which agitated the sea last winter, drove several large whales on shore on the French coast: the Class directed the information which they received on this subject to be examined by a committee consisting of MM. Lacepede, Geoffroy Saint Hilaire, and Cuvier.

These naturalists have remarked, that several of these animals were little if at all known, and that the subject, as interesting to the commerce and fisheries of France, deserved the attention of Government. They have given a description of the species cast ashore in great numbers near Saint Brieux: M. Lemauot, naturalist and apothecary of that place, having carefully collected all the essential parts, it was easy to discover among them a kind of dolphin, which had escaped the attention of all the methodical naturalists, and of which there was only one very bad figure in Duhamel's Treatise on Fishes. Its head is distinguished by a globular form, almost similar to an antique helmet. In length it was nearly 20 feet.

[To be continued.]

LX. *Intelligence and Miscellaneous Articles.*

ANTIQUITIES.

The admirers of Grecian antiquities will hear with pleasure that an important discovery has lately been made in Peloponnesus. The *Zante Gazette* gives the following particulars:—"Many artists and foreigners, lovers of the fine arts, had obtained permission to search in the temple of Apollo, situated in Mount Cotylius, in Arcadia. This search led to the complete frieze of the interior of the temple, composed of reliefs in marble, with nearly 100 figures, each more than two feet in height, and very little injured."

GALVANISM.

Mr. Singer has recently constructed an Electric Column of twenty thousand pairs of zinc and silver plates; its electrical effects are powerful, but it has not the slightest chemical agency. Pith ball electrometers diverge considerably by its actions; sparks are also produced, and jars charged with facility The plates in this extensive apparatus are small; a series of three thousand have been constructed larger; and the effects of these, though proportionably less, are more promptly produced. In a late course of lectures, Mr. Singer compared the results of the above apparatus with different Voltaic batteries excited by various menstrua; one of the batteries employed consisted of a thousand pairs of plates; another of four hundred, and a third of 64 pairs; the surfaces increasing as the numbers diminished: some curious effects were produced, apparently proving as Mr. S. stated, that the electrical and chemical powers of the Voltaic battery are distinctly separate phænomena.

LIST OF PATENTS FOR NEW INVENTIONS.

To Benford Deacon, of Cross Street, Islington, in the county of Middlesex, gentleman, for his improved method of applying air for domestic and manufacturing purposes, and of employing therein improved fireplaces and bricks.—13th March, 1813.

To William Hedley, of Wylam, in the county of Northumberland, coal-viewer, for his mechanical means of conveying carriages laden with coals, minerals, merchandize, and other things. 13th March.

To Richards Edwards, of the parish of Budock, in the county of Cornwall, doctor of physic; and William Williams, of the borough of Penryn, in the same county, surgeon, for their

their certain process for extracting arsenic from any of the ores or other substances in which it is contained, in a purer state than it is at present procured in this kingdom.—15th March.

To George Dodd, of South Villa, Wandsworth, in the county of Surry, engineer, for his certain improvements in umbrellas, which render the same more portable and convenient.—16th March.

To William Robert Wale King, of Union Court, Holborn Hill, in the city of London, tin-plate-worker, for his certain improvements in the application of heat to the purposes of boiling water, and other fluids, and to other useful purposes; and of the apparatus for performing the same.—22d March.

To Colonel William Congreve, of Cecil Street, Strand, for his mode of constructing the locks and sluices of canals, basons, or docks; and generally for transporting of floating bodies from one level to another.—23d March.

To Thomas Brunton, of Cooper's Row, Crutched Friars, in the city of London, merchant, for his discovered improvements in making or manufacturing of ships' anchors and windlasses, and chain cables or moorings.—26th March.

To John Hughes, of Poplar, in the county of Middlesex, excavator, for his improved method or apparatus for raising gravel or earth from the bottom of rivers and pits, and for screening and delivering the same into barges or other receptacles.—27th March.

To John Heathcoat, of Loughborough, in the county of Leicester, lace manufacturer, for his certain improvements on, and additions to, a machine for the making or manufacturing of bobbin lace, or lace nearly resembling foreign lace, for which he obtained a patent dated 29th day of March 1809; and that such improvements will make the machine more perfect and complete.—29th March.—Two months to inroll specification.

To David Thomas, of the parish of St. Mary Redcliff, in the city and county of Bristol, brightsmith, and ivory black manufacturer, for his method for burning animal bones for the purpose of extracting the greasy or fat property therefrom, and likewise for extracting the spirituous quality therefrom, and for reducing the remaining or dry parts of bones into a substance sufficiently prepared for being ground into ivory black; all which objects are obtained by one process only; namely, burning by fire.—30th March.—2 mo.

To Robert Hall, and Samuel Hall, of Basford, in the county of Nottingham, bleachers and cotton spinners, for their

their machine for the dressing, getting up, or finishing frame-work knitted goods manufactured from the stocking frame, whether consisting of hose, socks, caps, mits, gloves, or of any other kind or description whatever; and whether made of cotton, lambs' wool, Vigonia wool, silk, mohair, or any other vegetable or animal substance whatsoever, or any intermixture of these substances one with another.—30th March.—2 months.

To Joseph Egg, of Charing Cross, in the county of Middlesex, for his method of applying and improving locks.—30th March.—2 months.

To John Bennett, of the parish of St. Michael, in the city of Bristol, cabinet-maker, for his metal dove-tail joint applicable to portable and other furniture, and any kind of frame-work requiring strength and durability.—7th April.—2 months.

To James Timmins, of Birmingham, in the county of Warwick, manufacturer of sashes and hothouse lights with metal bars, for his improved method of making and erecting hothouses, and all horticultural buildings, and also the making of pine pits, cucumber lights, sashes and church windows.—7th April.—2 months.

To Robert Lewis, of Birmingham, brass-founder, for his method of making of brass (or of any other metal of which the component parts are copper and zinc) chimney pieces, or chimney-piece frames, plain or ornamented, either cast or of rolled metal, mounted on any other substance of which the outward mouldings or frame and inward pilasters shall be composed of such metal.—13th April.—2 months.

To Charles Plinth, of Temple Street, in the city of London, gentleman, who in consequence of communication made to him by certain foreigners residing abroad, is become possessed of various improvements in the construction of a vessel, machine, cylinder, reservoir, or fountain, (which he denominates "The Regency portable Fountain") used in the manufacture of water simply impregnated with fixed air or carbonic acid, and of artificial mineral and soda waters, and in the delivery of the same therefrom, and also in the delivery of cyder, perry, and other liquids.—13th April.—6 months.

To John Rangeley, of Oakwell Hall, near Leeds, in the county of York, gentleman, for his method of constructing and working engines or machines for lifting or raising of weights, turning of machinery of all descriptions, drawing carriages on railways, and capable of being applied to all purposes where mechanical power is required.—13th April.—6 months.

To

To Robert Campion, of Whitby, in the county of York, merchant, for his improved method of making and manufacturing double canvass and sailcloth with hemp and flax, or either of them, without any starch whatever.—13th April.—2 months.

To Charles Augustin Busby, of New Millman Street, in the county of Middlesex, architect, for his certain methods of constructing locks of canals, docks, and navigations, and of constructing improvements for locks of canals, docks, and navigations already existing, by means of which the loss of any quantity less than the whole quantity of the water now lost when vessels of any description pass locks constructed after any of the present known methods, will be prevented.—14th April.—2 months.

To Richard Coupland and Frederic Coupland, both of Leeds, in the county of York, manufacturers, for their manufacture of shawls, cords, Brunswicks, ribbed and plain kerseymeres, and milled cloths, from mixture of animal and vegetable wool, prepared and spun into yarn without oil.—28th April.—2 months.

To Joseph Hamilton, of the city of Dublin, gentleman, for his improvements on or additions to machines for making bricks, tiles, and earthenwares.—28th April.—6 mo.

Meteorological Observations made at Clapton in Hackney, from April 8 to April 28, 1813.

April 9.—Fair with a few *cumuli*, &c. but nearly clear, and very dry.

April 10.—Fine clear dry day, the wind NE. very early; soon after a SE gale arose, *cirri*, *cumuli*, &c.

April 11.—Quite cloudless all day; the blue sky not very deep though wind N. and then SE. Thermometer as low as 32° in the night. Barometer high.

April 12.—Misty morning, clear day, mist again in the evening, detached features of *cirrocumulus* at 11 P.M. Barometer 30. 25. Therm. 48°.

April 13.—Clear with some *cumuli* in the morning, and some *cirri* in confused lines, that is, none of them at all angular; very dry and clear in the afternoon. Wind N and E. Barom. 29 38. Maximum of Therm. 65. Minimum in night 35.

April 14.—The same kind of clear dry morning, afterwards flocky, elevated kind of clouds were spread about which seemed going into a state of *cirrocumulus*, faint features of *cirrostratus* and *cumulostratus* below them also appeared. Max. of thermometer 65°. Barometer 30. 48. *Cumuli* all disappeared before six, when it became cloudless.

April

April 15.—Fine clear day, with small yellowish or rather copper-coloured *cumuli*, a sort which accompany hot weather, which approach to the nature of those which precede storms, and are contrasted to the large white or silvery kind which appear in the intervals of cold snow showers. Maximum of thermometer 67°. Barometer fallen to 30 17. Features of *cirrus* strewed aloft in the afternoon with dark-coloured *cumuli* and *cirrostratus* in the evening indicated a change of weather.

April 16.—Much cloud in moving *nimbus* appeared in many places, with rocklike and tuberculated *cumuli, cirrus*, &c. By sunset the lofty *cirri* and *cirrostrati* refracted rich red colours. In the NW bars of alternate red and yellow were made by alternation of strata of *cirrostratus* and common haze between them. Wind westerly. Therm. max. 65°.

April 17.—A great deal of cloud in different stations in the morning, with the dense appearance of *nimbi* here and there; but the afternoon turned out fine, with *cirrus, cumulus* and *cumulostratus*. Therm. 64°. Wind WSW. Barom. 29, 80.

April 18.—Clear and warm day; *cumuli, cirri*, &c.

April 19.—Fair warm day. Therm. 65°. *Cumuli* and *cirri* with occasional *cumulostratus*.

April 20.—Clouded early; then fair, with some *cirrus* aloft with *cumulostratus* and *cumulus* lower. Wind calm from the NW. Therm. 69°. Barom. 30, 20 in the morning, but it fell somewhat in the day. Night clear; a falling star in direction to NW.

April 21.—Fair; the *cumuli* seemed smokelike and ill-defined; the evening became very cold. Therm. at 11 P.M. 34°; it was down at the freezing point at night.

April 22.—Cold wind from N.; some showers of rain and hail in the day. Clear night.

April 23.—Cold wind from NNW, and showers; with the usual phænomena of loose kinds of *cirrocumulus, cirrus*, &c. in the clear intervals. Wind in gales.

It is remarkable, that lately many appearances indicating rain have been observable, but they did not end in actual nimbification till after the occurrence of cold weather on the 21st.

April 26.—Cloudy, with rain at times; fair evening.

April 27.—Rain, more or less, all day, and a slight increase of temperature. Wind southerly.

April 28.—Hard rain all the morning, and it scarcely held up all day. Wind got to the northward.

Meteoro-

Meteorological Observations made at Cambridge, from April 29, to May 11, 1813.

April 29.—Rainy morning, and cold, cloudy, and windy afternoon; with cold northerly gales. The thermometer at 11 P.M. was 42°.

April 20.—Cold, rainy, and windy morning; it became warmer in the evening, and held up; but the night was dark and calm; and thermometer at 11 P.M. 44°.

May 1.—Cloudy, with a good deal of small rain. Thermometer at midnight 46°. Wind easterly.

May 2.—Clouds and threatening rain in the morning; the day cleared, and was calm and warm, with large confluent *cumuli; cirrostratus* and red haze in the west in the evening. Thermometer at 11 P.M. 48°. In the day were occasional and gentle gales of wind from the north*.

May 3.—Overcast early, afterwards clear. About ten o'clock in the morning, looking up I noticed the following phænomena: large beds of *cirrus* of flimsy but fibrous structure, changed into larger and more discriminative cirrostratous lines, and afterwards into *cirrocumulus* whose *nubeculæ* were small ragged pendulous and confluent aggregates, which continually changed their forms; while others were added at one extremity of the bed, by an apparent deposition of mist which separated into rows, and lastly became *cirrocumuli* by subdivision: such phænomena appearing and disappearing continued above, while *cumuli* sailed along below with motions not uniform either as to direction or velocity. Through the day *cumuli* increased in number, and *cirrus* appeared above; in the evening the clouds were confused and in different altitudes with a hazy moon. Thermometer 2 P.M. 66°, at 11 P.M. 51°. Wind below easterly. Showers came on in the night.

May 4.—Warm morning with gentle showers, and nearly calm air in variable currents. A thunder-storm occurred about two P.M. the thermometer just after it was 63°. After one of the flashes of lightning the rain came down with redoubled violence, mixed with hail. This augmentation of the strength of the shower often succeeds a discharge of lightning †. Thermometer at midnight 55°.

* A thick and sudden fog of short duration happened near London this day at about one o'clock. At Cambridge there was only a mistiness in the middle of the day.

† In the formation of thunder-storms, I have noticed that where the rain actually begins to form and descend, the intensity of the blackness is not so great as where the *cumulostratus* is only going into a state of *nimbus*. If therefore the density is increased in the formation of drops of water, the blackness must depend on some other peculiarity of structure.

May

May 5.—Cloudy at times; warm and calm; in the evening some *cirrocumulus*, with much *cirrostratus* and smoke-like scud. Cloudy at night. Thermometer midday 65°; 11 P.M. 55°.

May 6.—Fine warm day, but cloudy and misty in the morning; after it cleared, the haze continued, and large masses of *cumulus* prevailed of mountainous appearance. Therm. at 3 P.M. 68°. In the evening much *cirrostratus*. Therm. at 11 P.M. 51°. Wind northerly.

May 7.—Clouded morning; various clouds; warm day when it cleared; thunder-storms about, and some rain fell here. Clear night, with thermometer at 5 P.M. 50°.

May 8.—Clouded early, with a mist like what in Cornwall is called the pride of the morning; fair day with *cumuli* and *cirrus* above; at night a faint *halo* round the moon. The large clouds did not evaporate or disappear till late *. Therm. at midday 67°. Midnight 53°. Wind variable and quiet.

May 9.—Warm still day, with much cloud in the morning, and an appearance of nimbification at a distance, fine red sunset when the clouds broke; at night large confluent cirrocumulative masses. Thermometer 3 P.M. 69°. Midnight 56°.

May 10.—Warm close day with thick haze, through which much cloud was seen, generally *cumuli*, which in the evening appeared copper-coloured through the mist. At night there were *cirrocumuli* in beds, at considerable altitude. Thermometer at 11 at night 55°.

May 11.—Overcast morning, and somewhat cooler. The swift (*hirundo apus*) made its first appearance†. In the course of the day much cloud. No sun at times, and showers in the evening. Therm. at 11 P.M. 56°.

* If the nocturnal descent of the watery particles depend only on their comparative gravity being increased by a diminution of calorific repulsion, and the consequent uniting of the particles into minute drops of water; and if their reascent in the morning depend on a correspondent increase of levity by the acquisition of calorific repulsion; it would follow that water was much more expansible by heat than airs; since, by an alteration of temperature they were made at times much lighter, and at others heavier, than the particles of air.

† The swallow (*hirundo rustica*) was seen in the middle of April; and the martlet (*hirundo urbica*) at the latter end of April. It would be well if meteorologists would notice the earliest appearance of migratory birds in their journals, as the irregularities in their appearance may be dependent on atmospheric causes.

Corpus Christi College, Cambridge,
May 11, 1813.

THOMAS FORSTER.

METEORO-

METEOROLOGICAL TABLE,
By Mr. Cary, of the Strand,
For May 1813.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
April 27	45	50	40	29·65	0	Rain
28	40	45	40	·50	0	Rain
29	40	45	39	·65	30	Cloudy
30	39	48	40	.65	0	Rain
May 1	44	50	45	·67	0	Rain
2	47	54	50	·82	26	Cloudy
3	52	64	55	·86	46	Fair
4	57	63	56	·98	40	Showery
5	56	66	55	·98	52	Fair
6	49	63	56	·92	47	Fair, in the evening remarkable vivid lightning and loud thunder.
7	56	65	57	·78	52	Fair
8	57	68	50	·67	66	Fair
9	56	66	49	·65	54	Cloudy
10	57	67	54	·88	48	Showery
11	57	68	55	·68	50	Fair
12	57	68	55	·67	60	Fair
13	56	66	55	·68	59	Fair
14	58	64	50	·51	56	Fair
15	57	60	51	·60	48	Showery
16	56	59	50	·50	56	Stormy
17	55	63	51	·88	60	Showery
18	48	56	52	·77	27	Rain
19	55	64	56	·75	32	Fair
20	56	61	50	·45	20	Showery
21	55	50	44	·56	0	Hail-storms
22	48	56	50	·54	27	Stormy
23	50	62	54	·52	46	Showery
24	51	59	50	·49	48	Showery
25	54	61	51	·70	61	Fair

N.B. The Barometer's height is taken at one o'clock.

Erratum.—In the article "On the Aurora Borealis," p. 263, *for* "inclined from the zenith 13 degrees," *read* 18 degrees.

LXI. *An Attempt to determine the definite and simple Proportions, in which the constituent Parts of unorganic Substances are united with each other.* By Jacob Berzelius. *Professor of Medicine and Pharmacy, and M.R.A. Stockholm.*

[Continued from p. 346.]

XIV. Potass.

A. *Separation of the Base of Potass* [Potassium] *by means of the Electrical Column.*

I have employed in these experiments the same electrical column as in those which have already been published; it consists of 26 pairs of zinc and copper plates soldered together, each ten inches square, and their surface consequently containing 100 square inches each. Between the plates were placed pieces of pasteboard, dipped in a saturated solution of common salt.

I was for a long time in the habit of performing the decomposition in a glass tube, one end of which was closed round a wire of platina, which projected somewhat within it. I poured quicksilver into it, so that it stood above the level of the wire; and upon this a saturated solution of caustic potass, containing some undissolved crystals; I then brought the platina wire of the positive pole of the column into the alkaline solution, and made a communication between the fixed platina wire of the tube, and the negative pole of the column. While the column was in strong action, that is, commonly, for the first two days, the potass and the water were decomposed together; but when the action was weaker, the potass alone was decomposed. Since in this apparatus the affinity of potassium for oxygen appeared to be weaker than that of hydrogen, I thought that it merely depended on the too great intensity of the discharge, that any water was decomposed by it; and the cause of this intensity seemed to be the too small dimensions of the discharging surfaces in comparison with those of the columns, and with the quantity of electricity displaced. I therefore hoped that an extension of the discharge to a greater surface would diminish the intensity, and thus prevent the decomposition of water, and the consequent waste of electricity, and afford a larger quantity of potassium, since the whole electricity would be confined to this object. According to these ideas, I altered the apparatus; I poured quicksilver to the height of about a line into a little dish of glass, with a flat bottom, and about two inches

inches in diameter, and on it the solution of potass; I introduced the iron wire of the negative pole into the quicksilver, and brought a spiral wire of platina, which communicated with the positive pole of the column, into the solution, at the distance of about a line from the surface of the quicksilver: its coils were nearly in one plane, and parallel to the surface of the quicksilver: a plate of platina would have been less fit for the purpose, since its lower surface would have become continually covered with bubbles of oxygen, which could not have escaped. In this apparatus the decomposition of potass took place very rapidly; and in 24 hours the quicksilver, which weighed about 80 grammes, or $2\frac{1}{2}$ ounces troy, was so impregnated with potassium, that it was no longer fluid. It is evident that by a greater number of plates the intensity of the charge might have been so much increased, that this enlarged surface would still have been too small for the decomposition of the potass only.

Since in the operation of the column upon a saturated solution of potass, when quicksilver forms the negative conductor, the affinity of potassium for oxygen appears the weaker, it seems to follow that potass alone should be decomposed when the force of the column is infinitely small. In order to examine this point, I constructed a column with 20 pairs of zinc and copper plates, $1\frac{1}{2}$ inch in diameter, placing between them pieces of cloth moistened with a solution of salt. When I exposed a saturated solution of caustic potass to the operation of this column, in the apparatus first described, the positive wire emitted oxygen in small quantities, while no extrication of gas was observable at the surface of the quicksilver. At the expiration of six hours, a globule of quicksilver taken out of the apparatus already exhibited evident traces of potassium; and after 24 hours I found the quicksilver strongly impregnated with it, so that it caused an extrication of gas in pure water for several hours.

What has hitherto been mentioned, relates more to the physical properties of the electrical column than to the decomposition of the alkalis: it deserved however to be noticed, as a caution to those experimenters, who have the command of a column of large plates, in order that they may be aware of the danger of failing to obtain the greatest possible effect in saturated solutions, like those of the caustic alkalis, by employing too small a surface in the immediate operation of decomposition: while in the decomposition of the alkaline earths, as will be seen hereafter, another mode

of

of proceeding is required. The greater the surface of the plates, the greater must be the extent of the decomposing surface. Each point of it possesses indeed a less intensity of the electrochemical operation than in a smaller surface, but the sum of all the decompositions is greater. There is however for every given magnitude of the plates a maximum of the extent of the decomposing surface, beyond which the effect will not be increased. If the two surfaces are not parallel, the intensity of the discharge is increased in those points which are nearest to each other, and at the same time the sum of all the effects is diminished. If the battery acts powerfully, a vegetation of potassium shoots up from the negative conductor opposite to these points, but does not come into contact with the positive conductor, before the battery is weakened, and the evolution of gas from the positive wire, which has kept it at a distance, is diminished. If now the potassium touches the positive conductor, it discharges the column without any further decomposition, until the potassium is again oxidated, and converted into potass.

The quicksilver acts, in all these experiments, a very remarkable part. Its affinity to the base of the alkali has so great a share in the decomposition, that with the column which I have mentioned, taking every possible precaution, I have never been able to separate the component parts of potass without its assistance. I was very much surprised to find that even in Davy's battery, which was nearly thirty times as powerful as mine, the alkaline earths only afforded their bases distinctly when quicksilver was employed. I was first induced to employ this method in order to collect the very minute portions of metal which are set at liberty at the negative conductor, and dissipated by the evolution of gas: it was some time after this, that I observed, that the quicksilver operated also by its affinity, as will hereafter be shown by a direct experiment. When the negative wire is taken out of the quicksilver, the decomposition of the potassium ceases together with the negative state of the quicksilver, and the wire emits, as long as it continues in the alkaline solution, only hydrogen, without the slightest trace of a separation of potassium. Here then the affinity of hydrogen to oxygen appears to be the weaker, the decomposing operation of the electricity on the fluid being no longer strengthened by the affinity of quicksilver for potassium.

While the quicksilver contains no more than $\frac{1}{300}$ of potassium, it remains fluid; but afterwards, that part of it

 which

which combines with a greater portion, crystallizes, and swims on the rest. If the operation of the column is powerful, crystals are formed, which are small, irregular, and sometimes needle-shaped, and commonly vegetate towards the positive conductor, when hydrogen begins to be evolved, and the decomposition of potass to be diminished, if we neglect to push down the vegetation into the general mass of the quicksilver. As the power of the column is exhausted, the crystallization becomes more regular, and sometimes very large hollow cubes are formed, consisting of large quadrangular funnels, exactly like those of common salt. If these are collected, crushed into pieces, dried on blotting paper, and exposed, in a closed vessel, to a temperature of 50° Cels. [122°], they melt, and harden in cooling into a crystalline crust, consisting of small solid cubes, exactly as happens in the hasty evaporation of a small portion of culinary salt. When treated with water, their mass loses ·0127 of its weight, and consequently contains little more than $1\frac{1}{4}$ per cent. of potassium.

If we distil an amalgam of potassium in a small apparatus filled with dried hydrogen gas, over the flame of a spirit lamp, at first pure quicksilver passes over; but afterwards, when the metals remain in nearly equal volumes, some potassium accompanies it; and lastly, when nothing more passes over at a low red heat, there is found left in the retort a melted metallic substance, which when cold adheres so firmly to the glass, that the retort must be broken in order to obtain it. In the flexure of the retort some congealed drops are always found, perfectly resembling an amalgam of lead or tin. The residuum has a faint metallic splendour, is of a gray colour, inclining to red, and changing in a short time in the open air into dark brown or black: it is by no means pure potassium, this substance being, according to Davy, fluid at a moderate temperature, like quicksilver, but greatly resembles the protoxide of potassium, which Davy obtained by melting dry potass with potassium. When thrown into water, this substance sinks immediately to the bottom, and hydrogen gas is evolved with the greatest violence; at last a globule of quicksilver remains, which occupies $\frac{1}{20}$ only of its original volume. If this residuum is heated in the flame of a candle, it swells and changes into a saline mass, but does not inflame. I have proposed to myself three questions respecting this substance, neither of which I can satisfactorily answer: Is it a combination of the protoxide of potassium with quicksilver, that is, of a metal with an oxidated substance?

stance? Or, can so small a quantity of quicksilver be sufficient to disguise so completely the properties of potassium? Or, are both substances in the form of protoxides? When I had kept a portion of this substance about a month in a small stopped bottle, I found it surrounded by a gray brown cracked crust, in the middle of which a centre of the amalgam of potassium was found, containing so much quicksilver that it was completely fluid. I took off the gray brown crust, and threw it into water, in which it occasioned a very brisk extrication of gas. When moistened with a drop of water, it evolved hydrogen with the greatest violence, with heat and smoking. The water contained potass, and left behind yellow oxide of mercury. The gray brown crust therefore contained again a combination of potassium with quicksilver, in a condition of which I cannot form a distinct idea. During the most violent extrication of hydrogen gas, the quicksilver exhibited itself in the highest degree of oxidation. Can this be explained by an electrochemical polarity within the fluid? I think scarcely; for the effect is the same upon glass, as upon platina or wood.

Since, according to Davy's account, a greater heat, than that in which I distilled the amalgam of potassium, destroys and perforates the glass, I give up the hope of obtaining potassium pure by means of my electrical battery.

An unsuccessful attempt to separate the base of ammonia from a boiling solution of sal ammoniac by means of Rose's or D'Arcet's fusible mixture of bismuth with zinc and tin, induced me to attempt to collect potassium by means of the same compound. I had hoped, that if the former base could be collected in it, it would be easier to separate it from water when cold, and to distil it, than the amalgam with quicksilver; but in this hope I was disappointed. I employed in this experiment a glass tube, in the lower end of which a platina wire was cemented. On this wire I put the metallic compound, I poured on it the concentrated alkaline solution, and melted the metal by means of a lamp, which was kept burning throughout the experiment. The battery acted powerfully, and a considerable extrication of gas took place, both from the metal and from the positive wire. The solution became more and more saturated, and after two or three hours it began to dry up. I now hastily poured the liquid metal on a dry and cold saucer; and it immediately hardened. I wiped from its surface the potass which adhered to it, and scraped off some particles of the metal. When laid on a piece of litmus paper, reddened by

the vapour of vinegar, they did not restore its colour: when thrown into boiling water, they melted, without the extrication of any air; nor did the water become in any degree alkaline. Was the temperature, or the want of affinity of the compound for potassium, the cause that none was obtained? I at first suspected the former, and therefore exposed, in the same apparatus, a solution of potass, with quicksilver, to the heat of the same lamp. The quicksilver at first emitted much gas, but its quantity continually diminished as the solution became more concentrated by the effect of the heat: and the quicksilver, when poured out, was found to be strongly impregnated with potassium. The elevated temperature had consequently not prevented the decomposition of the potass in this experiment, but only in the beginning reduced the solution to the state in which it is found at common temperatures when less concentrated; and this experiment affords a positive proof that the quicksilver, in the decomposition of potass, operates not merely by collecting the product, but also by an affinity, which is wholly wanting in Rose's fusible compound.

B. *Attempt to ascertain the Composition of Potassium.*

It would be a fruitless labour to endeavour to add any thing to the excellent essay of Davy on the properties of the bases of potass and soda, even if it were possible to obtain these substances as readily as he did without the assistance of mercury. On the other hand, his attempts to investigate the proportions of these compounds seem to require some confirmation; since they were made on too small a scale, and the weight of the bases burnt was ascertained indirectly, so that small errors may have had a very material influence on the results.

I have attempted to perform the analysis of the alkalis in such a manner as to obtain a result on which more dependence may be placed, notwithstanding the many difficulties to which my method is liable. I suffered a portion of the amalgam of potassium of known weight to oxidate itself in water; I saturated the potass thus obtained with muriatic acid, and fused the salt that was formed. From the weight which the mercury had lost, I inferred the weight of the potassium, and from the analysis of the muriate of potass that was obtained, that of the potass formed. In the beginning the experiments disagreed very much among themselves; and in order to discover the cause of the difference, I was obliged to repeat the same experiment 20 or 30 times. The *first* cause of the uncertainty

was

was the small quantity of potassium contained in the amalgam; for I have often been obliged to take 60 grammes, or more, in order to work with one-third of a gramme of potassium. If now the larger weights are uncertain even to $\frac{1}{10000}$, we may easily obtain, when they are changed after the extraction of the potassium, a result too great or too small by a milligramme, which is of some importance in the calculation. I therefore always employed, in weighing the amalgam, some small weights, which amounted to little more than the potassium I expected to find, in order to be able to make as few changes as possible. The different degrees of dryness of the amalgam, before and after the experiment, may be a *second* cause of error. I therefore left the amalgam, which I obtained, in a small well stopped vessel, of which it filled four-fifths, for some time, on a very hot sand-bath, so that all the water adhering to it was decomposed by the potassium; I then poured the pure mass into a little phial, which it filled to the neck, and weighed it. After the extraction of the potassium, I dried the mercury again in a strong heat, so that it was completely freed from water. The amalgam must be weighed in a *stopped* [Sw. not "dried" as G.] vessel, otherwise it acquires weight during the operation, by the formation of drops of a solution of potass on its surface. An error may arise in the *third* place from the different effect of the solvent. If the amalgam was oxidated in pure water, the hydrogen escaped without the slightest smell, even when the oxidation proceeded pretty rapidly: but when I added muriatic acid, the hydrogen acquired a strong smell, resembling that which is perceived during the solution of zinc in this acid. Consequently the gas must have held something in solution; and this could be nothing else than potassium: hence the experiments, in which the muriatic acid was used for the solution of the potassium, always gave a smaller result than the rest. The same circumstance occurs when an amalgam of the base of one of the earths is dissolved in diluted muriatic acid; even if instead of the acid we only add sal ammoniac; while in this case such an addition is the more necessary, in order to obtain the earth in a state of solution.

The numerous experiments which I have made, respecting the component parts of potass, gave at first the proportion of oxygen varying from 16 to 20 per cent. I shall here only adduce those which were conducted with the greatest care, and of which the results agree tolerably well with each other. They indicate a greater proportion of

 oxygen

oxygen than Davy found, although most of the causes of error tend to lessen the apparent quantity of oxygen.

1.) I collected several portions of the amalgam, and weighed them before and after the extraction of the potassium. The potass obtained was mixed together and saturated with muriatic acid, the excess of which was evaporated in a small glass vessel, and the salt, together with the washings of the glass, was dried in a small golden crucible, weighing about three grammes, and then melted and weighed in the crucible. The whole of the potassium had weighed ·4575 gr. and the melted muriate of potass ·8675. Now the muriate of potass contains 64·19 per cent. of potass, consequently the ·8675 gr. answer to ·5568 gr. of pure potass; and potass consists, according to this experiment, of 82·166 of potassium, and 17·834 of oxygen. Some chemists have objected to this mode of determination, that the fused salt might possibly still contain some water. But not to mention, that the calculation for this salt gives almost the same quantity of potass, (see *Lärb.* i. kem. I. 399.) it is well known that in a melting-heat neither the muriate of potass nor that of soda is altered by charcoal, by phosphorus, or by iron, which would necessarily happen if they contained water, at the expense of which these combustible bodies would be oxygenized.

2.) The different operations of weighing might have occasioned inaccuracies, which might be singly unimportant, but of material consequence when added together. I therefore repeated the same experiment with a single portion of amalgam, which weighed 30·0775 gr. It gave, by treatment with water, ·1275 gr. of potassium; and this, saturated with muriatic acid, boiled, and melted, ·25 gr. of muriate of potass, containing ·160 gr. of pure potass: whence we have 80 of potassium to 20 of oxygen.

3.) The difference of these results being very considerable, I repeated the experiment with a greater quantity of a hardened amalgam, which weighed 67·003 gr. It lost ·32 in water, and gave ·608 of fused muriate of potass, which is equivalent to ·39027 of pure potass. Hence 100 parts of potass contain 82 of potassium and 18 of oxygen.

According to these experiments, 100 parts of potass seem to contain about 18 parts of oxygen, and 82 of potassium. And if we examine this result by the rules investigated and developed in the first part of this essay, we shall find it very correctly confirmed. The sulphate of potass consists, according to Bucholz's experiments on precipitation (Scherer x. 396) of 45·34 of the acid and

53·66

53·66 of potass, with one part of water; or 100 parts of sulphuric acid are saturated by 118·35 of potass. Now, if the 100 parts of sulphuric acid require in the 118·35 of potass, according to the foregoing analysis, 20·29 of oxygen, 100 parts of potass must consist of 17·152 oxygen and 82·848 of potassium.

From five grammes of melted muriate of potass, dissolved in water and precipitated by nitrate of silver, I obtained 9·575 gr. of fused horn silver. Rose obtained from 100 grains of this muriate 191½ of horn silver; which exactly agrees with my experiment. Consequently the *muriate of potass* consists of

Muriatic acid ..	55·81	100
Potass	64·19	179

Now, if 100 parts of muriatic acid suppose in these 179 parts of potass 30·49 of oxygen, 100 parts of potass must consist of 17·03 oxygen and 82·97 of potassium.

The difference between the results of the calculation and of the experiment amounts to somewhat less than one per cent. and I have good reasons for considering that of the calculation as the more accurate. Consequently *potass* consists of

Potassium ..	82·97	100·000
Oxygen	17·03	20·525

XV. Soda.

The bases of potass and soda are, according to Davy's excellent investigation, but little different, consequently they must be affected nearly in the same manner by the operation of collecting them in quicksilver by means of the electrical pile. The most material differences that I have observed are the following.

a.) The caustic soda is less readily decomposed than the potass, since the solution of soda is not so easily concentrated, and crystallizes sooner.

b.) The amalgam of sodium does not crystallize, and the appearance of the quicksilver is little changed, until it is strongly impregnated with it: but then the sodium forms sharp and silvery vegetations, which, as the proportion of the sodium to the mercury increases, assume a leaden gray colour, and the form of a cauliflower, exactly like the base of ammonia, which attaches itself to an iron wire covered with amalgam at the point. In the open air, its surface becomes moist much sooner than that of the amalgam of potass; and as soon as one portion of the solution of soda has been wiped away, another appears in increasing quantity.

tity. This circumstance renders the analysis of soda still more difficult, since the amalgam can scarcely be put into the vessel in which it is to be weighed, without an increase of its weight by the moisture which it acquires. Hence the results of my experiments on soda agree still less with each other than the foregoing, although they were performed with equal accuracy, and founded on the same bases.

1.) From 28 grammes of amalgam, by digestion with water and a little muriatic acid, which does not here, as in the case of potassium, produce a fetid hydrogen gas, I obtained ·1386 for the quantity of sodium. The soda afforded ·365 gr. of fused common salt, which indicates ·198 gr. of dry soda; whence we have for 100 parts of soda exactly 70 of sodium and 30 of oxygen.

2.) From 37 gr. of amalgam I obtained ·175 of sodium, whence ·46 gr. of fused salt were formed, containing ·2496 of pure soda. According to this experiment, soda contains 70·11 of its base, and 29·89 of oxygen.

3.) From 76 gr. of amalgam I obtained ·439 of sodium, which afforded 1·118 gr. of fused salt; containing ·6066 of pure soda. Hence we have, for 100 parts of soda, 72·37 of sodium and 27·63 of oxygen.

The last experiment having been performed with the largest quantity of the substance, and at the same time with the greatest accuracy of which analyses of this kind are susceptible, I think it probable that its result comes nearest to the truth. Several other experiments, performed with smaller quantities, gave proportions of oxygen varying from 27 to 36 per cent. It seems superfluous to describe them more particularly, as they are in all respects less to be depended on than this. I always found the proportion of oxygen apparently the greater as the quantity of the base, with which I worked, was smaller.

If we *calculate* the composition of soda in the same manner as we have done that of potass, we shall find here a similar agreement.

Bucholz obtained, from 1000 grains of crystallized sulphate of soda, 698 of sulphate of baryta, and he attributes to the salt 568 grains of water of crystallization. Consequently 100 parts of dry sulphuric acid must require for their saturation 82·09 of dry soda. But several calculations for different salts, proceeding with these numbers, convinced me that there must be some error in them, which probably arises from the uncertainty respecting the dryness of the Glauber's salt, and the quantity of its water of crystallization, the latter not being capable of so accurate a determination

determination in a salt which falls to powder, as is required for such experiments as these.

I therefore dissolved five grammes of ignited sulphate of soda in water, and added to it nitrate of baryta; the precipitate, when ignited, weighed 8·2 gr. answering to 2·789 gr. of sulphuric acid. In a second experiment I obtained from the same quantity 8·16 gr. of ignited sulphate of baryta. According to Bucholz's experiments, I ought to have had but 8·125 gr. The difference is not great, but sufficient to cause a considerable variation in the results. According to this experiment, the *sulphate of soda* consists of

Sulphuric acid ..	55·76	100·00
Soda	44·24	79·34

Five grammes of ignited muriate of soda, dissolved in water, and precipitated by nitrate of silver, gave 12·23 gr. of fused horn silver. Rose obtained from the same quantity, 12·175 gr. These 12·23 gr. of horn silver answer to 2·287 of muriatic acid; hence the *muriate* of soda consists of

Muriatic acid ..	45·74	100·000
Soda	54·26	118·627

These analyses of the two salts may be submitted to a test in the following manner. According to these experiments, 100 parts of muriatic acid are saturated by 179 of potass, and by 118·627 of soda: on the other hand, 100 parts of sulphuric acid require 118·35 of potass and 79·34 of soda for their saturation; but 179 : 118·63 = 118·35 : 78·43; a result which agrees tolerably well with the experiment, but shows that even in these four experiments there must be some error, which causes the difference.

The 100 parts of sulphuric acid suppose, in 79·34 of soda, 20·29 of oxygen, that is 25·56 per cent. with 74·44 of sodium: and 100 parts of muriatic acid require, in the 118·627 of soda, 30·49 of oxygen; whence 100 parts of soda must consist of 25·71 oxygen and 74·29 sodium.

These experiments do not indeed agree so well together as in the case of potass; but still sufficiently to enable us to conclude that we have in some degree approximated to the truth. As I have no reason to prefer one of these results to the other, I shall assume ("in round numbers") for the component parts of *soda*,

Sodium ..	74·29	(74 Sw.)	100·00
Oxygen ..	25·71	(26 Sw.)	34·61

Mr. Davy assigned, in his first investigations, to potass $\frac{1}{7}$ of oxygen, to soda $\frac{2}{9}$, that is, 14 and 22 per cent. respectively. In one of his letters he writes to me, "I have examined the composition of soda and potass on a pretty large

large scale, and found, that when pure metals are used, the potass contains about 15 per cent. of oxygen, the soda 23 to 27." Consequently his determination agrees, especially with respect to soda, pretty well with mine. [And in the last Bakerian Lecture we find that 100 parts of potassium absorb 18 of oxygen, and 100 of sodium 34; affording pure alkalies in a state of extreme moisture. *Gilbert.*]

XVI. Ammonia.

It would be useless to relate here all the fruitless experiments which I performed, nearly in Davy's manner, in order to obtain the base of ammonia in a separate state. It is utterly impossible to dry an amalgam of this basis, which is formed in a fluid. I therefore endeavoured to obtain it by the operation of dry bodies. For this purpose I mixed dry amalgam of potass with dry sal ammoniac, finely powdered, in a tubulated retort, provided with a receiver. Both vessels were previously filled with hydrogen, which I had caused to pass through a long tube filled with fused muriate of lime. The sal ammoniac began, after some time, to be decomposed, and the retort, in an hour and a half, was filled with an amalgam of the consistence of butter. When I wished to distil the mass, the amalgam subsided into the original volume of the mercury; and when the apparatus was opened, ammoniacal gas and hydrogen gas escaped, with a slight explosion. The neck of the retort was full of drops of water. This result is easily explained when we consider that sal ammoniac contains water of crystallization, amounting, according to the analysis hereafter to be related, to 19 per cent. The potassium is oxidated at the expense both of the water and of the ammonia in the salt, and this latter is reduced to a metallic form: but it is converted again at the expense of the water of a neighbouring portion of the salt, into ammonia; so that after the completion of the whole operation, only the oxygen of the water has vanished, having been employed in the formation of the potass, by which the sal ammoniac has been decomposed as a salt.

In order to separate the newly formed amalgam from the powder of sal ammoniac adhering to it, I made an instrument of a glass tube, at the ends of which I blew two bulbs, one of them running into a long and thin point: it was filled at the temperature of the freezing point, under boiled quicksilver, with dried hydrogen gas, passed through the point, which was then sealed, and stuck through a thick cork, which had been previously fitted to a bottle, in which was

was a large quantity of the amalgam of ammonium, prepared from sal ammoniac and amalgam of potassium. The bottle was opened, the point quickly broken off, and the cork made air-tight in the neck of the bottle, in such a manner, that the point of the tube entered the amalgam. The ball was then warmed, and the hydrogen forced into the bottle; as it cooled, the amalgam began to force itself into the tube, but it was of such a consistence that it stuck half way within the tube. By the application of warmth it acquired a greater degree of fluidity, but was still driven back. After repeated trials, I at last succeeded in getting a part of the semifluid mass into the ball; but it soon became covered with a thin coat of saline dust, so that I was obliged to content myself with the smaller portion of amalgam, which I had been able to collect in tolerable purity. When I had melted together the sides of the tube a little above the bottle, I tried to distil the amalgam contained in it from one bulb into the other. The saline powder, which had entered with it, lay on the metallic surface as a gray mealy covering. The mass was first heated over the flame of an oil lamp; the saline powder was thus still further decomposed, and the amalgam swelled to twice its volume: the salt in the mean time became agitated, and was converted into a fine snow white dust of muriate of potass. The amalgam was now almost entirely hardened, and during a whole hour, while the heat of the lamp acted on it without interruption, and raised its temperature far above the boiling point of water, it underwent no further change whatever. I then put a spirit lamp under the bulb: the mass then became black, and was covered with a dark crust, while the quicksilver returned to its original dimensions. By continuing the process, this crust disappeared, and the quicksilver was distilled over to $\frac{2}{3}$ of its volume. The product of the distillation was not quite so fluid as pure quicksilver; but the difference was inconsiderable. When I opened the apparatus under water, after it had been cooled, the quicksilver ran out, the water forced itself in, and occupied somewhat more than $\frac{2}{3}$ of the receiver; a proof that there was some ammoniacal gas in the apparatus, having been left in the gaseous form, unaltered by the amalgam remaining in the retort. The quicksilver, which ran out, gave indistinct marks of an evolution of gas, which I could however by no means attribute to the passing over of the base of ammonia; since this base, when the amalgam subsided, had certainly oxidated itself at the expense of the water in the powder of sal ammoniac. The amalgam left in

in the bulb of the retort was crystallized. When brought near a glass stopple, moistened with muriatic acid, it exhibited no more vapours: consequently the base of ammonia had been completely destroyed in this experiment.

Having learned, from this result, that the base of ammonia cannot be exhibited pure by experiments with the amalgam of ammonium, I performed several experiments in different ways, in order to determine whether and in what form this base can be exhibited separately. But I have hitherto been able to obtain no satisfactory determination of the question.

I have related, in the description of my electrochemical experiments, performed in common with Dr. Pontin, that the base of ammonia, combined with a small quantity of mercury, forms a leaden gray flocculent amalgam, which floats on water. This amalgam may be obtained without the immediate operation of electricity, if we mix the residuum left after the distillation of the amalgam of potassium, with a concentrated solution of sal ammoniac, in a strong and well stopped vessel. The decomposition begins immediately, and the newly formed amalgam swells sometimes to 150 or 200 times the volume of the quicksilver, which remains after the complete oxidation of both the bases. It emits at first a little gas; but this extrication of gas soon diminishes, and becomes at last, as the pressure of the air in the vessel increases, totally imperceptible. The amalgam then floats on the surface of the fluid, in the form of a porous, round mass, putting out vegetations on all sides. If we open the vessel, hydrogen gas is forced out with an explosion, and the newly formed amalgam begins to be decomposed, with a violent hissing. If the experiment is performed in an open vessel, the base of ammonia is oxidated almost the instant that it is produced.

Some attempts to combine the amalgam of the base of ammonia with sulphur, or with phosphorus, afforded me no satisfactory results. When, for example, I shook the amalgam of potassium with the sulphuretted hydroget [or hydrotheate] of ammonia, I obtained nothing but the sulphuretted hydroget [hydrotheate] of potass, and the usual buttery amalgam. Sulphuret of potass had no perceptible effect on the amalgam of ammonia, although the mercury alone would have blackened it; but when the amalgam shaken with sulphuret of potass, after being washed with pure water, was put into a solution of lead, it afforded evident marks of containing a little sulphur; which however did

did not appear to have the slightest effect on its external characters.

It is totally impossible to investigate the quantity of oxygen in ammonia by direct experiments on the amalgam of its base; I shall hereafter proceed to relate some experiments made in order to ascertain it. The whole of our knowledge of the base of ammonia, that problematical and yet in every respect highly interesting substance, consists almost entirely in our being assured of its existence under certain circumstances.

[To be continued.]

LXII. *On Mr.* Bennet's *Electrometer. By* Ez. Walker, *Esq.*

To Mr. Tilloch.

Dear Sir,—I do not remember to have seen any further account of the properties of Mr. Bennet's Electrometer, than what the inventor has given us in the Philosophical Transactions for 1786, which relates mostly to its extreme sensibility, in distinguishing small quantities of electricity. This instrument, however, has other properties, which merit the attention of electricians.

An excited surface being brought near the top of this instrument, but not so near as to produce as park, the gold-leaves will diverge in the same state of electricity as the excited surface; but as soon as it is removed, the gold-leaves will collapse, and instantly diverge again in a contrary state; and these changes will take place every time that the excited surface is moved to and from the cap of the instrument.

There is no work on electricity with which I am acquainted, that takes notice of these phænomena, nor was it till after I had made many experiments that I could form any thing like an explanation.

But after I found that there is a positive and a negative point, at every interruption of an electric circuit, or that the top plate of the instrument is negative, at the same time that the gold-leaves are positive, the phænomena no longer appeared inexplicable.

Place a slip of leaf-gold, about half an inch long and one-tenth of an inch broad, upon the top of the instrument, and let one end of it be fixed to the plate, with paste, gum-water, or varnish; then, if a glass tube, excited by rubbing it with silk, be brought near the top of the electrometer, the

the slip of leaf-gold will stand erect, being attracted by the excited tube; which shows that the top plate is possessed of the resinous or negative electricity; and the gold leaves within the glass will at the same time diverge with the vitreous or positive electricity, the same as the excited tube.

But as soon as the excited surface is removed, the gold-leaves will collapse and instantly diverge again, and when examined will be found to have received the resinous electricity, the top plate still possessing the same fluid.

Now, as part of the vitreous fluid has been repelled from the cap through the gold-leaves and tin-foil into the earth, the cap must necessarily possess less of the vitreous than its natural share; and consequently, when the excited tube is removed, the resinous fluid in the cap will attract the vitreous out of the gold-leaves: but this being too small a quantity to restore the equilibrium, the cap will still continue in a negative state, and communicate the negative or resinous fluid to the gold-leaves, which will cause them to diverge a second time; and as the cap is insulated it will continue electrified for some time after, if the instrument be a large one *.

This experiment also shows, that electricity by induction or position does not vanish as soon as the excited surface is removed, though Professor Robison and other writers on electricity are of a contrary opinion.

I am, dear sir,

Your obedient servant,

Lynn, May 27, 1813. E. Walker.

LXIII. *Case of Hydrophobia cured in India by Bleeding. By* John Shoolbred, *M.D. From the Supplement to the Calcutta Government Gazette, June* 8, 1812.

[Concluded from p. 365.]

REMARKS.

On hearing that a recovery from hydrophobia has been effected in the short space of two hours, by the single remedy of blood-letting, a doubt may probably occur to a person acquainted with the previous history of this formidable malady, and the nearly uniform failure of all attempts hitherto made for its cure; whether the disease now said to be cured, was in reality a genuine case of hydrophobia,

* The glass of an electrometer for these experiments should not be less than four inches in diameter, and nine or ten inches high.

produced

produced by the bite of a rabid animal. I admit the scepticism to be reasonable; for, in the relation of a case which has terminated so differently from all others yet on record, (not even excepting the case so successfully treated by Mr. Tymon,) it is natural to suspect either some misconception or misrepresentation of facts, or some fallacy in the deductions derived from them.

An attentive perusal of the preceding narrative will, it is presumed, remove these doubts from the minds of the majority of readers. Yet, as some individuals may not be convinced by that evidence which to others appears full and satisfactory; and as it is a matter of the utmost importance to future sufferers from hydrophobia, that no doubt should be allowed to remain, either as to the existence of the disease itself, in the case above related, or that the bleeding was the sole remedy, I shall, as briefly as possible, endeavour to establish the certainty of both those facts beyond the possibility of contradiction.

To a person who has never seen a case of hydrophobia, I acknowledge the difficulty, nay, almost the impossibility, of conveying by words an adequate notion of the disease. The horrors of that state must be seen to be fully conceived; but being once seen by a medical observer of any discernment, they are indelibly fixed in the mind; and I contend that it would then be highly improbable that he should ever mistake any other disease for hydrophobia; or take hydrophobia for any of those affections to which it has been said to bear some resemblance;—so deep and so permanent, I am convinced, would be the impression left on his mind by the contemplation of even a single case of hydrophobia. But when I state that my situation as surgeon to the Calcutta Native Hospital, for the last eighteen years, has afforded me opportunities of seeing the disease, which have fallen to the lot of few individuals in any country, and that no less than seventeen or eighteen cases of it have come under my observation within that period, in all of which both my diagnosis and prognosis (with the single exception of the latter in the case under consideration) have unhappily been but too fatally verified, it is not, I trust, laying claim to too great a share of discernment to assert that I could not easily be mistaken in a case of hydrophobia; and that I should consider my being so as unlikely, as that an experienced surgeon should ever confound two diseases the most opposite in their nature, because, to an uninformed eye, they might both exhibit something of the same external appearance.

Further: it has been usual with me, on the admission of a case of hydrophobia into the hospital, to send for some of my medical friends, not only that they might see a disease seldom occurring in private practice, but that I might have the benefit of their suggestions in regard to the treatment. On the present occasion, the promptitude necessary to the practice I had determined to adopt in the first case that occurred, and its astonishing effect in so suddenly and effectually subduing the disease, deprived me of the advantage I should now have derived in establishing the point in question from the concurring testimony of a judicious medical friend. But though not permitted to give direct evidence as to the existence of the disease in the case above detailed, these gentlemen can yet vouch, that they were never called by me to see a case of hydrophobia in which there existed the slightest doubt of the nature of the disease; and it will hardly be contended that I was more liable to mistake it in this case than on any former occasion.

If these facts and reasonings, combined with the account of the accident; the time that elapsed before the appearance of the symptoms;—the statement given by the patient of the commencement of the disease;—and by his friends, as to the state in which he appeared before he was brought to the hospital;—the symptoms under which he laboured when he arrived there;—should all be deemed insufficient to establish the real nature of the disease, I confess myself at a loss to conjecture what species of proof would be necessary for that purpose. The only defective point in the evidence appears to be our ignorance whether the dog by which Amier was bitten was actually mad or not? and though this cannot be proved by direct testimony, yet as it is known that the disease was prevalent among dogs, about that time, as will be hereafter noticed, it is presumed that this is an objection of very little weight. If therefore any individual, after duly considering all these circumstances, still continue in doubt as to the nature of the disease, may it not in conclusion be permitted to ask him what it was, if not hydrophobia?

That the disease, whatever it might be, was removed, and that almost instantaneously, by bleeding alone, admits, in my mind, of equally little doubt.

In Mr. Tymon's successful case, the symptoms only gradually disappeared, some of them remaining so late as the fourth day; and as opium, mercury, and antimony had been largely used during the whole time, and the patient's sytem

system was evidently under the influence of the mercury before he could be said to be free from the disease, an opinion might still be entertained, and actually was so, by many with whom I have conversed on the subject, that the cure was, after all, effected by the mercury, and not by the bleeding.

Dr. Berry himself, to whose rare and laudable zeal for the promotion of useful science, even at the period of closing a long and honourable career of public service, the world is indebted for the knowledge of Mr. Tymon's unprecedented case of success, alleges that the bleeding "saved Mason's life by diminishing violent action, *and admitting the effect of medicines that in all former experience had uniformly failed.*"

As this notion too corresponds with the most prevailing theory of the disease—though that theory has not in a single instance been verified by the success of the practice to which it gave rise, I consider it of great importance to correct it; lest, by still expecting some good from mercury and opium in hydrophobia, the attention of the physician should be diverted from a sufficient abstraction of blood,—on which, and on which alone, as far as a single case can prove any thing, the life of the patient seems entirely to depend.

That the first bleeding in the case above related, wholly though not permanently, removed every symptom of the disease, was proved, I presume, in the most ample manner, by the following six remarkable circumstances: first, the removal of the spasms; 2d, the freedom of respiration; 3d, the restoration of the power of swallowing fluids, and the absence of horror at their approach; 4th, the desire, instead of the abhorrence, of a current of air; 5th, the inclination for a natural alvine evacuation; and 6th, the power of sleeping.—All these unequivocal indications of recovery took place during or immediately after the first bleeding; and as none of them ever happened before to a patient in hydrophobia, except near the close of the melancholy scene, when they denote an entire sinking of the powers of life,—rather than the cessation of disease,—it seems but fair to ascribe them to a remedy which had never before been used as it was on this occasion—or, if so, unluckily not at the time when it was capable of doing good.

When a recurrence of the disease was threatened in two hours afterwards, the power of the remedy was again conspicuously manifested, and a second bleeding *ad deliquium* instantly stopped the progress of the symptoms, and,

 before

before a single particle of medicine of any kind had been given, permanently extinguished the morbid condition, whatever it may be, in which the essence of the disease consists.

These two points, therefore, appear to be fully proved; namely, that the disease was hydrophobia, and that the cure consisted in blood-letting alone.

But notwithstanding this unprecedented success, I am not so sanguine as to believe that venesection will cure every case of hydrophobia. It is probable that there is a period in the disease beyond which its curative effect cannot extend. What that period is, cannot be known without a more enlarged experience. But this very uncertainty affords only a more powerful reason for losing no time in resorting to the copious abstraction of blood, upon the very first appearance of unequivocal symptoms of the disease, as the delay of only a few hours may prove fatal to the patient.

In referring to notes which I have preserved of fourteen cases of hydrophobia, I find that eight of the patients died within six hours after admission. In these I cannot believe that bleeding would have done any good. But of the remaining six, who lived respectively 11, 13, 15, 20, 36, and 49 hours after admission, it is certainly reasonable to believe that it might have saved three or four. In a case so entirely hopeless, however, there could scarcely be harm to the individual, from trying it at any period of the disease. And as it is only by such trials that the real limits of its power can ever be ascertained to any useful purpose, it is rather desirable than otherwise that they should be made. One disadvantage however, eventually arising from such trials, requires to be guarded against. The medical profession, taught by innumerable disappointments, admit very cautiously the claims of any new mode of practice to general adoption. If several patients in hydrophobia, therefore, should happen to be bled in an advanced stage of the disease, and die,—as they inevitably would do whether they had been bled or not,—such cases would be quoted against the new practice as failures, and might tend so far to bring the remedy into discredit, as to prevent its being used even in cases where it might have proved the certain means of saving life.

I am the more desirous of noticing the unfavourable effect upon the adoption of the new practice, which may eventually arise from bleeding at too late a period of the disease, and of entering a strong caution against the hasty rejection of the

the remedy from such instances of failure, in consequence of the circumstance having very nearly happened to myself only three days before the occurrence of the case of Amier.

On Saturday evening, the second of May 1812, a native of Arracan employed in Calcutta as a cook was brought to the hospital labouring under symptoms of hydrophobia. I went to him that moment, with the full determination of putting in practice the plan that had succeeded in the hands of Mr. Tymon; but I found that the unfortunate sufferer had been ill, according to the account of his friends, for 56 hours. His pulse was imperceptible, his skin cold, and his features sunk. I therefore got him to swallow 100 drops of laudanum, which he effected, as frequently happens, with greater ease than is usual in an earlier stage of the disease; and I ordered an enema with 300 drops. The patient was dead in half an hour. Now what I wish to impress upon the mind of the reader is—that if, in this case, the disease had been somewhat less advanced, the pulse still perceptible, and the strength less sunk, I should certainly have bled the patient;—which at such a period could scarcely have prevented death: it would more probably have appeared to have accelerated that event; and, if so, might consequently have had the effect of preventing my pushing the bleeding in the case of Amier to the extent necessary to the cure. I must therefore here insist, that numerous failures in an advanced stage of the disease will form no just ground for the rejection of a remedy which has been so incontestably proved to have cured the disease when used at an earlier period. As well might the practitioner reject bleeding in the commencement of peripneumony or enteritis in a robust athletic patient, because in each disease there is a period after which the detraction of blood, so far from curing, would serve only to hasten the fatal event.

Nothing, however, can fix the real value of the remedy but experience. It is highly desirable that this may be speedily obtained; and as the disease does and must very frequently occur in this country, whether we possess the means of curing it or not, we cannot doubt that but a very short time will elapse without further trials of this remedy; and it may be presumed that the medical practitioners, who are so widely distributed throughout India, will fairly and circumstantially communicate to the public the result of their experience, whether attended with success or not.

It may be necessary to observe, however, that merely opening a vein and drawing a considerable quantity of

 blood

blood is *not* the practice. The vein must be opened by a large orifice, the blood quickly evacuated, and allowed to flow, without regard to measurement, *ad animi deliquium.* Nothing less than this is capable of at once arresting the progress of the disease, relieving the spasmodic affection of the heart and arteries, suppressing excessive sensibility and irritability; and, in short, of admitting the restoration of that due balance of action and influence, both in the circulating and nervous systems, on which the continuance of life and health seems to depend.

But I lay no stress on this or any other pathology of the disease. Well authenticated trials of the remedy in an early stage of it, are what I desire to see. If it fails in many of these, when used in the manner above proposed, within twenty-four, or, to speak with some latitude, thirty hours of the commencement of the symptoms, I confess I shall feel much disappointed; and not a little mortified, to be obliged, after such fair prospects, to reject a remedy, which has effected twice, in the short space of seven months, what was scarcely ever effected before; and to class it with that useless farrago of remedies and practices, which, though used hundreds of times, and for a series of years, have never once been known to accomplish a cure of hydrophobia.

With respect to the subsequent treatment of the patient, it is scarcely necessary to make any remark. The case clearly shows that for the hydrophobia no subsequent treatment was required. But as this and many other cases on record show a great disposition to disordered and excessive action of the liver, it may perhaps hereafter be found useful to administer mercury both as an evacuant, and to the extent of affecting the mouth, with or without opium, according to circumstances.

It is usual, when new and successful expedients are first promulgated, to wonder why they never were thought of before. In conformity to this habit, I have frequently within the last ten days been asked why, in a disease so often proved incurable by other means, bleeding was not before tried? The fact is, however, that *bleeding has often been tried.* But owing, probably, to the evacuation not being pushed far enough, when used in an early stage of the disease—or to the period for its beneficial employment having elapsed before it was resorted to—the relation of the cases in which it was used afforded little or no encouragement to further trials; while the theory that has prevailed for nearly a century in regard to the nature of the affection,

and

and its classification with diseases of the nervous kind, accompanied by great debility, tended directly to discourage all lowering plans of cure, and to point out antispasmodics and tonics as the only résource in hydrophobia.

Dr. Mead, who was very confident that he had found an infallible preventative of the disease, in *a little liverwort and black pepper*, aided by bleeding and cold bathing before the commencement of the course of medicine, says, "as to all other ways of curing the hydrophobia, I own I have not been so happy as to find any success from the many I have tried. Bathing at this time is ineffectual. *I have taken away large quantities of blood;* have given opiates, volátile salts, &c. &c. &c.—All has been in vain, *because too late.*" Notwithstanding his disappointment, he still concludes, "if any relief could be expected in this desperate state, I think it would be from *large bleeding even* ad animi deliquium, *before the fibres of the membranes have lost their natural force by convulsions.* But after all it will generally happen, that (as the Greeks said upon deplorable cases) 'Death will be the physician that cures.'" This, though a recommendation, was certainly no great encouragement to blood-letting.

The doctrines of Boerhaave also led him and his pupils to recommend and practise bleeding in hydrophobia. The celebrated Leyden Professor says, "the distemper is to be treated as one highly inflammatory, upon the first appearance of the signs which denote its invasion, by blood-letting from a large orifice, continued till the patient faints away; and soon after by enemata of warm water and vinegar," &c. &c. and he adds, "that this practice is supported by some small number of trials." But the particulars of this successful practice are not given.

I find, however, a trial of it at Edinburgh, more than 60 years ago, by the late Dr. Rutherford, a pupil of Boerhaave's, who took away *gradually* sixty-six ounces of blood from a patient who had already been bled the same morning. As this patient lived forty-eight hours after the large bleeding, it is probable that it was used somewhat early in the disease, and should, therefore, it may be said, have succeeded. Why it did not, it is impossible now to tell; but I am persuaded the circumstances attending its failure had great weight in deterring others from pursuing the plan recommended by Boerhaave, and in giving an entirely different direction to the practical views of physicians on the subject of hydrophobia.

On the failure of bleeding in this case, Dr. Rutherford, who

who then with great reputation filled the practical chair of the most celebrated school of medicine in Europe, candidly retracted an opinion which he had learned from Boerhaave, and which had directed the measures he took. He declared in his public lectures, that "he was convinced now, that the hydrophobia is a spasmodic and not a high inflammatory disease; that though bleeding may be useful in preventing furiousness, neither that nor the proper antiphlogistic method is to be depended upon as the proper cure of hydrophobia; that in such cases, after bleeding once or twice, he would order *sal succini*, *musk*, opium, and perhaps *blisters*." Thus, at once sending abroad into all parts of the world the opinion that large bleeding was useless in hydrophobia, and inculcating the use of antispasmodics only.

Dr. Cullen says scarcely any thing on hydrophobia, further than that his chief reliance would be on mercury. Macbride asserts that "Doctor Nugent was the first that pointed out the true nature of hydrophobia—which before his time was generally considered as an inflammatory disease. Dr. Nugent's patient was *largely blooded*, and took moreover large quantities of musk and cinnabar as well as opium; and toward the close of the cure opium was given along with camphor, musk, and assafœtida. *But the opium is what we are chiefly to rely on.*" Thus again withdrawing the attention of the practitioner from the large abstraction of blood, to which the cure in this case was most probably to be ascribed.

It is needless to multiply quotations to prove, that nearly the same opinion of the disease, and the remedies most applicable to it, have prevailed with little variation up to this day, with the single exception perhaps of Dr. Rush, who in consequence of his peculiar notions about inflammation, but which do not seem to be countenanced by the appearance of the blood drawn from hydrophobic patients, again inculcated the necessity of blood-letting.

Finding therefore so many authorities against bleeding in hydrophobia—and not a single cure ascribed to it, except those mentioned in a vague way by Boerhaave—it is by no means surprising, that it should for more than half a century scarcely even have been thought of as a remedy in this disease. I am aware that it has sometimes been used as an auxiliary, when the pulse has been full and the strength great, in order to render the patient more manageable. But as it has till lately never been employed as the remedy of sole dependence, nor applied in the manner necessary

necessary to produce a decided effect upon the disease, I confidently trust that its failure nearly up to the present day, will not be considered as militating against the expectation of success which I think we are now fairly entitled to entertain from its future employment.

It is at any rate highly encouraging to know, that, in the only three cases in which it has been trusted to as the principal or the sole remedy, it has succeeded to our utmost wishes.

The first case is that by Dr. Burton, in America, which was suggested by Dr. Rush's lectures, and was published about seven years ago in different periodical works. But unfortunately, in consequence of the case not being very accurately related, and its being combined with some fanciful theory, it does not appear to have been acknowledged as a clear instance of hydrophobia; and the benefit which might otherwise have been derived from it was wholly lost to the world. Whether it was actually a case of hydrophobia or not, is not now worth disputing, being in possession of Mr. Tymon's case, and of that which has given rise to these already too greatly extended remarks.

I cannot, however, conclude without saying a few words on the practices which have been principally in use up to this time. Never having seen Dr. Nugent's case, the only instance of well authenticated recovery from hydrophobia with which I was acquainted, previous to these three, is one related by Dr. Shadwell, in the Memoirs of the London Medical Society, in which, on the authority of a Greek manuscript, oil was used both externally and internally. Relying on this example, I gave oil a very fair trial in several of the first cases that fell under my care. But although I often got the patient to swallow a considerable quantity of it, and applied it frequently by enema, as well as to the skin by almost incessant frictions, it never appeared to do the least good. I therefore abandoned it.

I have subsequently used every mode of treatment that I have ever heard or seen suggested, with equally little success, except arsenic, which, though with no better hope, was to have been my next trial, had not Mr. Tymon's case fortunately occurred, to point out the practice which has already so well justified the confidence reposed in it.

On those occasions, besides the full trial given to oil, I used opium to a great extent, in every possible way; mercury, musk, camphor, blisters, galvanism, and enemata of laudanum and infusion of tobacco, all to no purpose. Nothing ever alleviated a symptom except the two last, which certainly

certainly did lessen the spasms; and therefore, when bleeding may hereafter be used too late to succeed, I would recommend them as remedies, capable, though not of preventing death, yet of allowing the fatal event to take place with less suffering to the unhappy patient than any thing else with which I am acquainted.

On the recommendation of Dr. Bardsley, of Manchester, a gentleman who has with unwearied zeal endeavoured to investigate the nature of hydrophobia with a view to the discovery of its cure, I also gave a very fair trial to volatile alkali. Contrary to all expectation, I succeeded in getting into the stomach no less than three drams of carbonate of ammonia made into boluses with crumb of bread. But the event was unhappily just the same as in all former cases.

Dr. Bardsley was led to this suggestion by the perusal of Mr. Williams's cases of recovery from the bite of *cobra de capello* by means of *eau-de-luce*, and he endeavours to recommend its adoption by the following observation: "surely in the treatment of so fatal a disease as canine madness, it is proper to adopt any method of cure founded on RATIONAL PRINCIPLES. *Analogy under these circumstances seems to be our surest guide.*"

It is melancholy to relate, that though hydrophobia has been unusually frequent in England of late years, and many cases of it have been treated by the most eminent practitioners in London, both in hospitals and private practice, yet not a single case of recovery is recorded. Dr. Parr, author of the Medical Dictionary, published for the express purpose of exhibiting the state of medical science up to the present time, after telling that every thing has been tried, and that every thing has failed in effecting a cure, consoles his reader by acquainting him with the infallibility of cutting out the part as a preventive, adding emphatically, in Italics: "*In short, full, effectual and* COMPLETE EXCISION *of the wounded part is the only certain means of relief*; AND THIS IS CERTAIN." But still leaving us in the same hopeless condition as to any means of cure after the disease has actually taken place.

Dr. John Hunter concludes a most able paper on the history of the disease, and the trials made for its cure, with these words: "after the symptoms of hydrophobia have appeared, no medicine or remedy that has hitherto been used has relieved, much less cured, the disease." And finally,

A well informed anonymous writer, in the Medical Annual Register for 1808, after presenting a sketch of the practice

practice that had been pursued in London during that year, and noticing the failure of every expedient, sums up his history with this opprobrious sentence: "On the whole, therefore, we may be considered as remaining in the most entire ignorance both of the nature of the disease, and of the method of cure, or even of palliating a single symptom."

Such was the disheartening language universally held on the subject of hydrophobia. I humbly trust that it can be held no longer; that the case above related, coming so soon after that of Mr. Tymon, entitles us to indulge more animating views for the future; and that it will not be long before additional experience shall serve to confirm the hope, which seems now to rest on so promising a foundation, that a remedy has at length been discovered for this hitherto uncontrollable disease.

It is mortifying to the pride of science to acknowledge it,—but if further trials of bleeding *ad deliquium* shall confirm its power of curing hydrophobia when used early in the disease, it is nevertheless impossible to conceal that this *solum et unicum remedium* has apparently been hitherto overlooked in consequence of an overweening fondness for system, which led medical writers to class hydrophobia with diseases of the nervous kind, and to dwell particularly on its resemblance to tetanus. That disease being considered as highly asthenic, blood-letting, perhaps without sufficient reason, has been thought inadmissible. Antispasmodics and tonics have been employed in all quantities and forms; and though by such remedies scarcely one case of tetanus in fifty has ever been cured, the same treatment has been rather preposterously it should seem, transferred to hydrophobia,—because, under such hopeless circumstances, *analogy has been said to be our surest guide*. Whither has it guided us? Never certainly to a single cure of hydrophobia.—It may perhaps with greater truth be said to have been an *ignis fatuus*, which has served to lead us into difficulties and dangers, rather than to conduct us into the salutuary path of curative science; and that, perhaps, in more diseases than the one under immediate consideration.

After expressing so little respect for analogy, the professed guide of physicians, in the treatment of hydrophobia, shall I not be accused of inconsistency, or of indulging in notions of too speculative a nature, if I offer a hint that some use may yet be derived from this favourite analogy, by pursuing it in an opposite direction; and if, instead of applying to hydrophobia the treatment which seldom succeeded

ceeded even in tetanus itself, we now transfer to tetanus, and perhaps to other diseases of the same kind, the practice which has been inconstestably proved, in two instances at least, if not in three, to have been successfully employed in hydrophobia?

Almost all authors have spoken of this analogy, and some have gone so far as to affirm that tetanus may be easily mistaken for hydrophobia. I confess myself to be of a different opinion; being fully persuaded that no person who has often seen both diseases could ever mistake the one for the other, and that for the following reasons: first, in tetanus the lower jaw is immoveably fixed, and the patient speaks by the motion of his lips only, with a hissing kind of noise;—whereas in hydrophobia the lower jaw is moveable to any degree; and is in fact, in the exacerbations, almost in perpetual motion, often resembling the action of hawking or retching, for the purpose of bringing forward and expelling the viscid saliva which constantly collects about the fauces;—and in the second place, that though the swallowing of fluids may be difficult or impossible in tetanus, and the attempt even accompanied with convulsions of the face, throat, and chest, yet the obstacle is confined to the actions connected with deglutition alone, and the name, the approach, and the touch of fluids, have never in my experience thrown the patient into the agony of horror, distress, and despair, which is invariably witnessed in hydrophobia.

Asiatic Mirror, May 20, 1812. J. S.

LXIV. *Description of an annular Saw, calculated to cut deeper than its own Centre. By Mr.* THOMAS MACHELL, *Surgeon, Wolsingham, near Durham*.*

SIR,—I TAKE the liberty to solicit you to lay before the Society of Arts, &c. an instrument which I presume will facilitate several operations in surgery, and which I have named an annular saw. It is particularly well adapted for the division of cylindrical bones, surrounded by muscles, blood-vessels or nerves, and with less injury to those parts than by any other instrument in present use.

In operations upon the cranium it has the superiority over the trephine, and Mr. Hay's saw, as it can be applied

* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, for 1812.——The gold medal of the Society was voted to Mr. Machell for this communication.

to

to the cranium in every form or posture, and remove any depressed portion of bone with the greatest safety and speed. Mr. Cline and Mr. Whatley have seen the instrument, and expressed their opinion that it would be found a very useful instrument in many operations.

My business as a surgeon, and pressing avocations in the country, prevent me from staying more than a few days in London. I should therefore esteem it an additional favour if the Society would soon take it into consideration, that I may personally explain its use.

The principle of this machine will be found useful for many mechanical as well as surgical purposes.

I am, sir,

Your humble servant,

London, March 24, 1812. THOMAS MACHELL.

To C. Taylor, M.D. Sec.

Reference to the Engraving of Mr. THOMAS MACHELL'S *annular Saw, which cuts beyond its own Centre.* Plate X. fig. 1, 2, 3, 4, 5.

Fig. 1, is a view of the saw, its frame, and the wheel-work which actuates it; fig. 2, is an edge view of it; fig. 3, is a view of part of the interior work, and figs. 4 and 5, a detached view and section of the saw itself.

In fig. 1, AB represents a solid arm or rod of iron, which supports the whole instrument: this rod is fitted up in such a manner, that it can be moved in any direction either to raise or lower it, to move it from right to left, or to lengthen it out endways, so that the joint B at the end of it can be placed in any possible situation within certain limits: this joint connects another piece D with AB, and at the end of this is a joint E, the motion of which is at right angles to the former joint, and it attaches the saw frame FG to it: this frame, see also fig. 2, contains a toothed wheel H, which is turned round by the handle I, and by its teeth actuates a smaller wheel concealed within the frame, but its dimensions are shown by the dotted circle described round the screw *a*, which is its centre pin: this wheel turns another, *b*, (see the section, fig. 3,) and this moves a third wheel, *d*, which has a circle of six pins, *c*, projecting from its face, and these turn round the saw K, by entering into notches made in its edge, so that it is actuated by its circumference instead of its axis, as is the case with ordinary *circular* saws; its construction is explained by figs. 4 and 5, in which K is the ring or annular saw, the

the inside of the hole through it being, as shown in fig. 4, turned out concave, in the latter rather larger than the edges, somewhat like the rim of a spectacle frame; then the internal circle M, being accurately fitted into it, is swelled or bulged out by means of a taper mandrell, driven into the hole in its centre to such a size that it will fill the outer ring or circle exactly, in the manner of fig. 4, and then it cannot get out of its place sideways, because the interior circle exactly fills the groove or hollow parts formed round within side the annular saw K: this internal circle M thus becomes the axis on which the saw turns. The circumference of the saw, as shown in fig. 5, is notched all round with fine teeth, which perform the cutting, and at intervals it is cut with deep notches, into which the pins on the face of the wheel *c* are received, and act upon the ring so as to turn it round: the interior ring M, or axis of the saw, is supported by being screwed to a piece of iron, N, which also carries the centre pin of the wheel *d*, and is itself screwed to the inside of the brass plates FG of the frame, by the screws shown in fig. 1. W is a handle to guide and direct the saw, moving it upon its several joints B and E into any required position; *o* is a spring which in certain positions balances the weight of the frame FG, &c. depending upon the joint B: P, fig. 2, is a gauge consisting of a flat slip of iron, PS, which is fitted to the underside of the frame; it has a groove formed in it through which a screw passes, and the nut Q will fasten it at any required point; it is moved sideways by the screw R, and adjusted to advance to any required distance towards the extreme end of the saw: its use is to regulate the depth to which the saw shall penetrate in cutting.

The very singular property of this annular or circular saw, in cutting deeper than its centre, renders it likely to prove of great utility in a variety of surgical and mechanical operations.

LXV. *On the Changes of Colour produced by Heat in coloured Bodies.* By M. GAY-LUSSAC*.

THE various colours presented by the different bodies of nature present variations in their shades, and frequently pass from one tint to another when they are exposed to a certain temperature. There would be nothing remarkable in these changes, if they were owing to a chemical alteration;

* *Annales de Chimie*, August 1812. No. 248, p. 171.

tion; but I shall only regard those here, which, being subordinate to the intensity of heat, cease to take place immediately when the temperature resumes its primitive state. I shall divide this memoir into two parts: the first will contain a detail of facts, and the second their relations with other phænomena.

First Part.

I ought to premise, that in the following experiments I have not taken an exact account of the temperature to which the coloured bodies have been exposed. I generally contented myself with heating pieces of porcelain upon burning coals, and afterwards throwing coloured bodies upon them: sometimes however I exposed them upon an earthen plate at the heat of the sand-bath, but never lower than 100°, or higher than 400°. Lest I should inaccurately define the various changes of colour produced by heat, I requested M. Merimée, who is well versed in the colour business, to be present at my experiments, and to write down the results with his own hand. I cannot do better, therefore, than faithfully republish the notes which he took upon that occasion.

Experiment 1.—*Chinese Vermilion.*

Its colour is not a pure red: it contains yellow. On exposing it to the heat of the sand-bath it became deeper, and assumed the carmine shade.

Experiment 2.—*Oxide of Mercury obtained by the Calcination of the Nitrate of Mercury.*

Its colour is orange. At the temperature of 100° it assumed a deepish red, and approached the colour of common cinnabar: at a stronger heat it became of a fine cinnabar red, and at a still stronger heat about 300° it passed to the violet colour, first assuming a blue colour. The colour of this oxide being orange renders it capable of passing to a brilliant red, but not to a fine violet *.

Experiment 3.—*Red Lead.*

It presents nearly the same phænomena with the oxide of mercury; but, as its orange colour is finer, heat makes it assume a more brilliant red. The violet colour which is developed in it afterwards is not finer than that of the oxide of mercury.

* When we pound this oxide of mercury, it takes an olive yellow shade deeper than its primitive colour, and which is developed the better, the more the oxide is pounded. This new colour gets deeper when heated, and becomes of a cinnamon colour.

Experi-

Experiment 4.—Nitrate of Cobalt slightly dried.

This salt, which when cold is of a wine red, becomes blue the instant its temperature is a little raised: when cooled it resumes its primitive colour, and thus passes successively from one tint to another, when we vary its temperature, independently of the influence of humidity.

Experiment 5.—Red Sulphuret of Arsenic, or Realgar.

When in the mass it is red: but when it is pounded it has an orange colour mixed with a chesnut red. On exposing it to heat, it takes the colour of colcothar.

Experiment 6.—Glass of Antimony.

It presents when pounded an orange yellow colour clouded with a good deal of gray. At a heat of about 400° it assumes a brownish red, as if it had been mixed with the red oxide of iron.

Experiment 7.—Oxide of Bismuth prepared by decomposing the Nitrate of Bismuth by Heat.

Its colour is a dirty white mixed with a little orange yellow. On gradually raising its temperature, it becomes of a very fine yellow, and passes progressively to the chesnut red: upon cooling it resumes its primitive colour. We ought to remark, that it does not pass by the pure orange: thus the red tint which it acquires is not a pure red, but that of the rust of iron.

Experiment 8.—Oxide of Tin prepared with Nitric Acid, and calcined.

Its colour is similar to flowers of sulphur sullied by a little gray. When heated it assumes a yellower shade with a little red.

Experiment 9.—Oxide of Zinc.

Upon calcining nitrate of zinc exempt from iron, an oxide was obtained which when cold is of a straw colour: a heat of about 300° gives it a colour like that of Naples yellow. At a higher temperature the shade is yellower, and compared with that of the chromate of lead is greener, and more intense.

Experiment 10.—Flowers of Sulphur.

The first stage of heat produced a more lively yellow, in which we remarked the gray tint which accompanies this colour. When they began to fuse, the shade became olive yellow, and the colour increasing it became red.

Experiment 11.—Yellow Sulphuret of Arsenic, or Orpiment.

We may regard this colour as the purest which mineral substances

substances can furnish. When exposed to heat it is at first orange, and afterwards takes the red colour of the oxide of iron.

Experiment 12.—*Turbith Mineral.*

Its colour is of a very fine yellow resembling the ranunculus; when heated it becomes of a brick-red colour.

Experiment 13.—*Chromate of Lead.*

When cold, it is of a very fine yellow mixed with a little orange. Upon heating it, it passes to the orange, but does not become so brilliant as might be expected.

Experiment 14.—*Muriate of Iron at the Maximum.*

Its natural colour when it is concentrated is of a fine yellow, but when applied in a thin layer its colour is that of broom flowers a little sullied. On raising the temperature it assumes a chesnut colour. Red therefore has been added to its primitive colour.

Experiment 15.—*Green Oxide of Chrome.*

When projected upon a piece of earthen ware almost red hot, it becomes of an olive colour, as if we had mixed a little colcothar with this oxide.

Experiment 16.—*Liquid Muriate of Copper.*

This salt was of a greenish blue similar to what is called water-green. When heated, but not so as to evaporate its water, it becomes of a fine green, as if it had been made brighter with gamboge: when cold it resumes its primitive colour. If we concentrate it more it retains, when cold, a fine green colour, containing however less yellow than when it is warm. On evaporating it again, it becomes of a dirty ochrey yellow. We may obtain this colour by mixing a little orange colour with the fine green colour which it had formerly.

Experiment 17.—*Highly concentrated Nitrate of Copper.*

When cold it is of a pure blue, and does not appear to contain any green. When heated it becomes of a blueish green, as if a little gamboge had been added to the solution.

Experiment 18.—*Azure.*

When exposed to a heat of 400° it is altered to gray, as if a little orange colour had been added to it.

Experiment 19.—*Protoxide of Copper.*

When cold it has a grayish tint mixed with brownish red, which makes a dirty violet with gray. With heat it

 becomes

becomes gray like charcoal powder: in this way it assumed a blueish shade.

Experiment 20.—Deutoxide of Copper.

Its colour is a deep gray containing a little brownish red. On exposing it to heat, it becomes blacker, which proves that it has taken from the blue which has neutralized the red and the yellow.

Experiment 21.—Deutoxide of Iron.

It is of a gray colour, retaining very little of the brownish red. Heat produces blue, and its colour becomes of a purer gray, which by opposition makes the former appear redder.

Experiment 22.—Peroxide of Antimony; Pearl Powder of Kerkringius.

Its colour is a bright white, like white lead. When heated it takes a slight shade of dirty yellow or yellowish gray. We obtain a similar effect with the volatile oxide of antimony, but in a feebler degree.

LXVI. *Researches upon the Heat developed in Combustion, and in the Condensation of Vapours. Read before the French Institute on the 24th of February and 30th of November 1812. By Count* Rumford, *F.R.S. Foreign Associate of the Imperial Institute of France, &c. &c.*

[Continued from page 297.]

§ V. *Heat developed in the Combustion of Spirit of Wine and Alcohol.*

As the constituent parts of these inflammable liquids may be regarded as well determined, by the results of M. de Saussure's excellent work, I undertook a second time to examine them, with a view to what were the quantities of the heat which are developed during their combustion. I had begun this business five years ago; but after having made a considerable number of experiments, I abandoned it on account of the great difficulties which I met with; but the instant I found means to make my apparatus more perfect I recommenced.

Before giving the details of my experiments, I ought to say a few words upon the difficulties which attended the enterprise, even since I possessed my new apparatus, and upon the means I used to surmount them. There were even dangers to which I was exposed, which it is necessary I should

I should detail, in order to caution those who undertake the inquiry.

When I made experiments with highly rectified alcohol, and particularly with ether, I found it very difficult to prevent a remarkable part of these volatile liquids from escaping in vapour from the mass of the liquid which remained in the lamp. I constructed a small lamp in the form of a round tobacco box, with a beak rising from the centre of the circular plate which forms the top: and upon this I fixed a small reservoir to contain cold water, intended to cool the beak, and prevent the heat from descending to the body of the lamp: but this precaution was not sufficient when I burnt ether, as I learned to my cost. Although the reservoir was twice as large in diameter as the lamp, and it was filled with cold water, this water was so heated in a few minutes that there was an explosion of ether in the state of vapour which took fire in the open air, and burned with a flame which touched the cieling, threatening to set fire to the house.

Rendered cautious by this accident, I constructed a new lamp, much smaller than the former: it was only an inch in diameter and three-fourths of an inch in depth, and its beak, which was only two lines in diameter, was three-fourths of an inch high. In order to keep this small lamp cool while it was burning, it was placed in a small tub, and kept constantly submerged three lines below the upper extremity of its beak in a mixture of water and pounded ice. These precautions were sufficient to prevent explosions, but did not prevent the evaporation of the ether or of the alcohol. I was convinced of this fact by observing, that always, when I made two consecutive experiments without filling the lamp afresh, the alcohol constantly appeared weaker in the second experiment than in the first.

It was not difficult to account for this phænomenon: the most volatile, and consequently the most combustible parts of this liquid being dispersed in vapours in the interior of the lamp, found means to escape at the beak, with part of the liquid which had passed through the wick, leaving the alcohol which remained in the lamp sensibly weakened.

In order to remedy this imperfection, I constructed a third lamp, which I have presented to the Class. It is made of copper, and has the form of a small cylindrical vase an inch and a half in diameter, and three fourths of an inch in height, swelled a little at top and hermetically closed by a stopper of copper, which being ground with emery is wedged into the neck of the vessel.

 This

This stopper is perforated at its axis by a small vertical hole, which is wholly closed or partly opened, when requisite, by means of a small vice with a copper nut.

A small pipe about a line and a half in diameter, and two inches six lines in length, issues horizontally from the sides of this vase, and very near its bottom. At the distance of an inch and four lines from the vase this pipe forms an elbow, and afterwards ascending vertically forms the beak of the lamp.

This small pipe is very thin throughout, except at its upper extremity, where it is thicker, in order to give it a form convenient for receiving a very small cylindrical extinguisher five lines high by three and a half in diameter, intended to close the beak hermetically, without touching or deranging the wick at the instant the lamp ceases burning, and to keep it constantly shut when the lamp is not burning.

Without this precaution, in experiments made with ether, so great a quantity of this volatile liquid would escape in vapour, by the beak of the lamp, during the time taken up in weighing it, that there would be no way of determining the quantity burnt.

To support the beak of the lamp, it is stayed by two pieces of copper wire, which proceed in a horizontal direction to join the body of the lamp to which they are soldered.

In order to keep this lamp constantly cold, as well as the liquid which it contains, it is placed in a small tub and entirely covered, excepting the extremity of its beak and that of its mouth-piece, by a mixture of pounded ice and water.

When we weigh the lamp, it is taken out of its tub, and care is taken to wipe it well with dry linen before placing it in the scales.

When the lamp is lighted, we must not forget to open a little, and but a very little, the vice which forms its stopper, after it has burned two or three minutes; for without this precaution it might go out.

As the small horizontal pipe, by which the liquid which is burnt passes from the reservoir of this lamp to reach its beak, is always filled with the liquid so as to have no communication with the vapour of the liquid which is dispersed in the upper part of the reservoir, this vapour can no longer escape by the beak of the lamp, as it did before I contrived the method of preventing it.

If I have given a very minute description of this lamp, it appeared to be necessary to spare those who wish to repeat my

my experiments, or to make others similar, all the difficulties which I had to surmount before discovering the means of governing the combustion of inflammable liquids which are very volatile.

As the apparatus which I used in my experiments is now well known, it will be easy to follow their details. and appreciate their results. I shall endeavour to describe them with precison, but at the same time as rapidly as possible.

After laying in a stock of common spirits of wine and alcohol of different degrees of purity, I determined with the greatest care their specific gravity at the temperature of 60° (Fahrenheit), taking that of the water at the same temperature =1000000. I made choice of this temperature, in order to determine afterwards with more facility the quantities of water which each of these liquids ought to contain, according to the tables which were formed from the results of the experiments of M. Lowitz.

By the following table we shall see the specific gravity of each of these liquids, and the quantity of pure alcohol of Lowitz, and of water, which it contains.

Kind of Liquid.	Specific Gravity at 60° F.	Composition.	
		Pure Alcohol of Lowitz.	Water.
Alcohol at 42°......	817624	0·9719	0·0821
Alcohol of commerce	847140	0·8057	0·1943
Spirits of wine at 33°.	853240	0·7788	0·2212

Such are the results of the experiments which were made to determine the quantities of heat which these liquids furnish in their combustion.

In three experiments made with spirits of wine, the quantities of heat manifested were:

In the first.... 53·260 pounds of water at the freezing point, carried to ebullition.

In the second.. 51·727 pounds.

And in the third 52·855

Mean result... 52·614 pounds.

As one pound of this liquid contains only 0·7788 pound of alcohol regarded as pure by Lowitz, the other parts =0·2212 pound, being water only which does not burn: in order to see how much water at the freezing point could be boiled by one pound of pure alcohol of Lowitz, we have only to divide the quantity which is the measure of the mean heat developed in the experiments with spirits of wine, by the fraction which expresses the quantity of the

 alcohol

alcohol which is found in one pound of this liquid; then $\frac{52\cdot614}{0\cdot7788}=67\cdot558$ pounds, which is the measure of the heat developed in the combustion of one pound of *pure alcohol of Lowitz*, according to the mean result of the experiments made with spirits of wine.

In two experiments made with common alcohol, I had for the mean result 54·218 pounds of water boiled; and as this alcohol contained 0·8057 pound of pure alcohol, this will give for the measurement of the heat developed in the combustion of one pound of pure alcohol of Lowitz $\frac{54\cdot218}{0\cdot8057}$ $=67\cdot293$ pounds of water heated 180° Fahrenheit.

In three experiments made with alcohol at 42° which had a specific gravity of =817624, I had as the mean result 61·952 pounds of water heated 180° F. with the heat developed in the combustion of one pound of this liquid.

According to this result, one pound of pure alcohol of Lowitz ought to furnish a sufficiency of heat in its combustion to raise the temperature of 67·57 pounds of water to 180° of Fahrenheit, for it is $\frac{61\cdot952}{0\cdot9179}=67\cdot101$.

On taking the mean between the results of the eight experiments which were made with these three alcoholic liquids, we shall have for the measure of the heat developed in the combustion of one pound of pure alcohol of Lowitz, 67·317 pounds of water at the temperature of freezing carried to ebullition.

It will be very interesting without doubt to know if this quantity of heat agrees with the quantities of combustible matters (carbon and hydrogen) which exist in this alcohol: this is precisely what we shall see.

According to the analysis of M. de Saussure, one pound of alcohol of Lowitz contains

Carbon	0·4282	pound
Free hydrogen	0·1018	
Water	0·4700	
	1·	

Now, according to the estimate of Crawford, we shall have

	Pounds of Water heated to 180° Fahrenheit.
For the measure of the heat in the combustion of 0·4282 pound of carbon	24·667 lbs.
And for the measure of that which is furnished in the combustion of 0·1018 pound of hydrogen	41·738 lbs.
Total	66·405
The experiments yielded	67·317

It is rare in so delicate an investigation to find so perfect an agreement between the results of the experiments and those of the calculation.

§ VI. *Heat developed in the Combustion of sulphuric Ether.*

I have already mentioned the difficulties which I overcame before being able to regulate the combustion of this substance in such a way as to render the results of my experiments regular and satisfactory; but I met with still further difficulties in the course of this delicate inquiry.

As alcohol is necessarily employed in making sulphuric ether, and as these two liquids may be united in any proportions, it is extremely difficult, if not impossible, to separate them entirely; and as both are colourless and limpid, either when mixed or separate, we can scarcely judge of the degree of purity of the ether, except by its specific gravity, and even in this way but very imperfectly.

The most highly rectified sulphuric ether which I could procure, and which I employed in my experiments, was prepared in M. Vauquelin's laboratory. Its specific gravity is 72834 at the temperature of 16° Reaumur. As that which was employed by M. de Saussure in his analysis was only of the specific gravity of 717 at the same temperature; by regarding the ether which I employed as being a mixture of the same degree of purity with that of M. de Saussure, and the pure alcohol of Lowitz having a specific gravity of 792, we shall find upon making a calculation, that the ether which I employed was a mixture of 85 parts of ether of the specific gravity of 717, and 15 parts of pure alcohol of Lowitz of the specific gravity of 792.

On burning this mixture under my calorimeter, after having brought my apparatus to the highest degree of perfection, I obtained the following results:

Duration of the Experiment.	Ether burnt.	Quantity of Water heated.	Temperature of Water in the Calorimeter. At the Commencement.	At the End of the Experiment.	Elevation of the Temperature of the Calorimeter.	Temperature of the Air.	Result. Quantity of Water heated 180° of Fahrenheit with the Heat developed in the Combustion of one pound of the Combustible.
mi. sec.	gram.	gram.	° F.				
11	1·96	2781	$55\frac{1}{2}$°	$65\frac{5}{8}$°	$10\frac{1}{8}$°	60°	79·996
11·15	2·01		$54\frac{1}{4}$°	$64\frac{3}{4}$°	$10\frac{1}{2}$°	60°	80·710
9	2		$58\frac{1}{4}$°	$69\frac{5}{8}$°	$19\frac{3}{8}$°	60°	80·146
20	3·29		$56\frac{3}{4}$°	$73\frac{3}{4}$°	17°	61°	79·884
22	3·06		$56\frac{1}{2}$°	$72\frac{1}{2}$°	16°	$64\frac{1}{2}$°	80·784
Mean Result of the five Experiments							80·304

Continuing to make use of the estimates of Crawford, for the quantities of heat developed in the combustion of hydrogen and carbon, we shall see if these estimates are sufficient to account for the heat manifested in these five experiments.

As the ether employed was a mixture of 15 parts of pure alcohol of Lowitz, and 85 parts of ether of the specific gravity of 717 at the temperature of 16° Reaumur, and consequently similar to the ether analysed by M. de Saussure, we shall begin by determining the quantity of heat which ought to to be developed in the combustion of these fifteen parts of alcohol.

As M. de Saussure has shown that in one pound of Lowitz's alcohol (of the specific gravity of 792) there are 0·4282 pound of carbon and 0·1018 pound of free hydrogen, we ought to find in 0·15 pound of this same liquid, 0·06423 pound of carbon, and 0·01527 of free hydrogen.

According to the estimate of Crawford, 0·06423 pound of carbon ought to furnish a sufficiency of heat in its combustion to raise the temperature of 3·7002 pounds of water to 180° Fah.; and 0·01527 pound of hydrogen ought to furnish enough to raise to the same temperature 6·2607 pounds of water; and these two quantities of water making together 9·9609 pounds, is the measure of the quantity of heat which must be developed in the combustion of the 15 parts of alcohol which are found mixed with 85 parts of ether, in order to form the combustible liquid employed under the name of sulphuric ether in my experiments.

Now, as one pound of this mixed liquid has furnished in its combustion enough of heat to raise to 180° of Fahrenheit 80·304 pounds of water; if we deduct from this mass the quantity of water which the 15 per cent. of alcohol must heat (=9·909), that which remains (=70·3431 pounds of water) will be the measure of the quantity of heat developed in the combustion of 85 per cent. of ether of the gravity of 717, which exists in this combustible liquid.

According to the analysis of sulphuric ether made by M. de Saussure, we ought to find in one pound of this liquid (of the specific gravity of 717)

Carbon	0·590 lb.
Free and combustible hydrogen	0·194
Oxygen and hydrogen in the proportions necessary to form water	0·216
	1·

Consequently, we ought to find in 0·85 pound of the same

same kind of ether, the following quantities of combustible substances; viz.

Carbon	0·5015 lb.
Free and combustible hydrogen	0·1651

We shall now see if these quantities of combustible substances are sufficient to account for the heat which is manifested in our experiments.

The 0·5015 pound of carbon ought to furnish sufficient heat to raise 28·89 pounds of water to 180° of Fahrenheit; and the 0·1651 pound of hydrogen sufficient to heat 67·64 pounds to the same degree.

These two masses of water form together 96·53 pounds; but we shall see that the quantity of heat furnished by the 85 parts of ether in the experiments cannot be greater than that which is necessary to heat 70·3431 pounds of water to 180° Fahrenheit.

As the experiments have been made with the greatest care, and frequently repeated, and always with very uniform results; and as the estimates which we have adopted, with respect to the quantities of heat which are developed in the combustion of hydrogen and in that of carbon, have been confirmed so as to leave little doubt upon this subject: upon investigating the cause of the great difference between the quantity of heat actually developed in the combustion of the 85 parts of sulphuric ether burnt in the experiments which we have examined, and the quantity given by calculation, we are compelled, in my opinion, to admit that there is an error in the analysis of this liquid, and that it does not contain so much free and inflammable combustible matter as M. de Saussure ascribes to it.

As it seems to me to be much more probable that an error has been committed in determining the quantity of free hydrogen in this substance than in determining the quantity of carbon, I shall suppose with M. de Saussure that there is really in one pound of sulphuric ether (of the specific gravity of 717) 0·59 of carbon; but instead of estimating the quantity of free hydrogen in this liquid according to the results of M. de Saussure, I shall adopt the estimate of Mr. Cruickshanks.

This excellent chemist concluded from his experiments, that in the vapour of sulphuric ether the carbon is to the hydrogen as 5 to 1.

In the 0·85 pound of sulphuric ether (specific gravity 717) which were mixed with the 0·15 pound of alcohol, in order to form one pound of the mixed liquid employed in my experiments, there were 0·5015 pound of carbon; and dividing

dividing this number by 5, we shall see that this carbon ought to be united with 0·1003 pound of free hydrogen, instead of being united with 0·1654 pound, as we shall suppose according to M. de Saussure.

Let us now see if, by adoping the analysis of Mr. Cruickshanks with respect to the hydrogen instead of that of M. de Saussure, the calculation will agree better with the experiment.

We have seen that the quantity of water heated to 180° Fahrenheit, which represents the quantity of heat which must be developed in the combustion of the 0·15 pound of alcohol, was .... 9·9609 lbs.

And that the quantity answering to 0·5015 pound of carbon, which exists in the 0·85 of ether, was .... 28·89

We shall for the present add that which answers to the combustion of 0·1003 pound of free combustible hydrogen, which, according to Mr. Cruickshanks, ought to be found united to this quantity of carbon in order to form the ether .... 41·123

These three quantities of water together are the measure of the heat which must be developed in the combustion of one pound of sulphuric ether of the kind employed in my experiments .... 79·9739

The mean result of five experiments was 80·304.

This coincidence between the calculation and the experiment is doubtless too remarkable to be owing to chance, but I am ready to prove that it occurred without being foreseen or expected.

From all these results we may conclude, that one pound of sulphuric ether of the specific gravity 717 at the temperature of 16° Reaumur, or of the same species with that employed by M. de Saussure, this liquid should have furnished in combustion enough of heat to raise to 180° F. 82·369 pounds of water; viz.

That furnished by ·059 pound of carbon 33·989 lbs.
And that furnished by 0·118 lb. of hydrogen 48·386

82·369

If the proportion of free hydrogen in the ether analysed by M. de Saussure was really such as he has determined it to be, one pound of this liquid ought to furnish a sufficiency of heat in its combustion to raise to 180° of Fahrenheit 113·566 pounds of water, viz.

That

That furnished by 0·59 pound of carbon	33·989 lbs.
And that which was furnished by 0·194091 of hydrogen	79·577
	113·566

But I can the less persuade myself that this liquid can furnish in its combustion so much heat, because one pound of white wax furnished no more than what was sufficient to heat 94·682 pounds of water to the same degree.

According to the analysis of M. de Saussure, 100 parts of sulphuric ether of the specific gravity of 717 at 16° Reaumur are composed of

Carbon	59 parts
Hydrogen	22
Oxygen	19
	100

Supposing that the 19 parts of oxygen are combined with 3·6 parts hydrogen, so as to form with them 21·6 parts water, 100 parts of this kind of ether ought to be composed of

Carbon	59
Free and combustible hydrogen	19·4
Consequently, inflammable substances	78·4
Water	21·6
	100

From the result of my experiments, 100 parts of this kind of ether ought to be composed of

Carbon	59
Free or combustible hydrogen	11·8
Consequently, combustible substances	70·8
Water	29·2
	100

Or, reducing the water to its elements:

Carbon		59
Hydrogen, free or combustible	11·8	
Ditto, non-combustible	3·5	15·3
Oxygen		25·7
		100

According to M. de Saussure's analysis as well as from the results of my experiments, 100 parts of pure alcohol of Lowitz,

Lowitz, of the specific gravity of 792, at the temperature of 16° Reaumur, are composed of

Carbon	42·82
Free or combustible hydrogen ...	10·18
Consequently, combustible substances	53
Water	47
	100

Or, reducing the water to its elements, 100 parts of this alcohol are composed of

Carbon		42·82
Hydrogen, combined and non-combustible	5·64	
Hydrogen, combustible	10·18	15·82
Oxygen		41·36
		100

By supposing that water exists completely formed both in alcohol and ether, the constituent parts of these two liquids would be, according to the results of our inquiries,

	Alcohol.	Ether.
Carbon	42·82	59
Combustible hydrogen	10·18	11·8
Water	47	29·2
	100	100

The elements of water exist most assuredly both in alcohol and ether; but there is good reason to believe that water does not exist in its natural state of condensation in these two substances, neither when they are in a state of liquidity, nor when, being sufficiently heated, they are transformed into elastic fluids.

When we mix water with alcohol, there is a considerable change both in temperature and volume, which indicates a new arrangement of elements, or a chemical action; and what proves in a still more certain manner that this action has taken place, the liquid which results from this mixture may be distilled, *i. e. vaporized* by heat, and afterwards *condensed*, without being decomposed: but it is above all in the little heat which is developed in the condensation of the vapour of alcohol and ether that we discover certain proofs that the oxygen and hydrogen which exist as elements in these liquids do not exist in the state of water. I shall recur to this subject again.

[To be continued.]

LXVII. *On*

LXVII. *On the Effects of Fumigations of Oxymuriatic Acid in neutralizing the pernicious Vapours which exhale from Burying-places**. *By* M. Girard, *Engineer, Director of the Water-Works at Paris†.*

When in 1784 the bodies were dug up which had been buried in the Cemetery *des Innocens* at Paris, those only were disturbed which lay three or four feet below the surface, but there were pits of more ancient formation lower down, the bodies in which were not yet consumed. An opening was made to the very bottom of one of these lower pits, in which the solid mason-work which supports the lower basin of the fountain of the Innocents was built. From this pit a most fetid smell was exhaled, which would have infected the whole neighbourhood if the apparatus of M. Guyton had not been resorted to. This apparatus was composed of four earthen pots, in which were mixed in the requisite proportions, sulphuric acid, oxide of manganese, and muriate of soda. The mixture was renewed every morning when the workmen began their labours, and every night when they left off, by which means the pots were left all night in the pit. Not only were the inhabitants thereby preserved from all annoyance, but none of the workmen, of whom there were one hundred, experienced the slightest accident, although the work was executed in the months of June, July, and August 1809.

In the beginning of the year 1812, the churchyard of the village of Claye, through which the canal of the Ourcq passes, was opened; and in consequence of the same pre-

* *Annales de Chimie*, tome lxxxiii. p. 281.

† The serious accidents produced by cadaverous emanations when burial-places are incautiously opened, are too well known. Dr. Haguenot has published some shocking instances, and no place has been more remarkable in this respect than the churchyard of the Innocents. It appears that during the latter years of its existence as a burial-place, no less than 3000 bodies were annually deposited there. Ever since 1724, the inhabitants of the adjoining houses had called the attention of Government to the dangerous effects of this great focus of putrid infection; and in 1765 they succeeded in obtaining a decree of the Parliament of Paris, ordaining its suppression, and the removal of all places of sepulture beyond the barriers. Notwithstanding all this, in 1781, the reports made by order of the Police, and presented to the Academy of Sciences and to the *Société de Médecine*, proved that the insalubrity of the atmosphere had so increased as to occasion repeatedly in the vicinity diseases of a putrid character, and that animal food recently prepared speedily underwent a fetid alteration, and that the walls of the cellars were so impregnated as to cause pimples in the hands of those who touched them, accompanied by excoriations, &c. These effects were attempted to be removed by throwing quick-lime to the depth of six inches into the pits, but in a few days the deleterious gas burst forth again.—Note of the Editors of the *Ann. de Chimie.*

caution

caution being used, no accident happened, and the inhabitants were not in the least incommoded.

The same disinfecting apparatus has been employed in the works going on in the Rue Montmartre. The quarrying for the sewers and drains has been carried down to the pits which were dug in this part of Paris in the reigns of Charles VI. and Louis XIII. The filth with which these ancient cemeteries was filled, exhaled an infectious and insupportable odour; but the process of Guyton being speedily applied, no accident happened.

LXVIII. *On the Relations of Air to Heat, Cold, and Moisture, and the Means of ascertaining their reciprocal Action. By* J. Leslie, *Esq. F.R.S.E. Professor of Mathematics in the University of Edinburgh* *.

"The various phænomena of heat are most easily conceived by referring them to the operation of a peculiar fluid possessing extreme activity, and diffused through all bodies." It constantly endeavours to maintain its equilibrium or equal diffusion among bodies, and its accumulation in any substance is generally marked by a corresponding expansion. The extent of this expansion in different bodies varies as they transmit heat more or less rapidly, or have divers conducting powers. Air is found, in like circumstances, to expand five times more than alcohol, 20 times more than mercury, 160 times more than platina, and even 580 times more than glass. The thermometer is an instrument contrived to measure its own expansions; but it can mark only the heat of its own bulb, as affected by external communication, and any further inferences drawn from its different indications are merely the result of some process of reasoning †.

"Heat combines with different substances in proportions widely varied, and depending in each on its peculiar and intimate structure. In general, it is more copious in liquids than in solids, and in the aëriform fluids than in liquids. But still the allotment among the different bodies, appears to be as various as their distinctive properties. Under similar circumstances, hydrogen gas will hold or absorb ten times as much heat, as an equal mass of atmospheric air; water twice as much as olive oil, and three times as much as

* Abstracted from "A View of Experiments and Instruments depending on the Relations of Air to Heat and Moisture."

† See Tilloch's Essay on Caloric, Phil. Mag. vol. viii.

as concentrated sulphuric acid; sulphuric acid, again, twice as much as glass; and glass itself, twice as much as silver, and five times as much as mercury. If a pound of water heated 30 degrees be poured into another pound of water at the temperature of the apartment, the surplus heat will become equally shared between the two masses, the infused portion losing 15° of its heat, and the recipient gaining 15°. But if a pound of mercury heated 30 degrees above the standard be poured into a pound of water; while both of these now acquire the same temperature, the mercury will lose 29 degrees, and the water gain only one degree. Hence, in the state of quiescence, mercury contains 29 times less heat than water, and has its temperature 29 times more affected by equal accessions of that elementary fluid. But even the same substance, if its form be mutable, will exhibit similar differences, according to the aspect which it assumes. Thus, ice is more easily heated than water, and water than steam. The same addition of heat which would raise the temperature of ice 10 degrees, would only raise that of water 9 degrees, and that of steam 6 degrees. At each stage of transition, there is hence an apparent pause, attended with a corresponding absorption or evolution of heat.

"Thus, if a vessel filled with ice be suspended over a steady fire, the ice will continue at the freezing point, till, perhaps in an interval of half an hour, it be entirely melted; it will then grow regularly warmer, till, after 40 minutes, the water begins to boil: nor will the temperature of the liquid now receive any further increase, the subsequent accessions of heat being wholly expended in the formation of the expelled steam, and which would require the space of three hours and a half. In the act of thawing, therefore, and again in the process of ebullition, there is a successive absorption of heat, amounting respectively to a difference of temperature in the water of about 75 and 525 of the centigrade degrees, or 135 and 945 on Fahrenheit's scale. But the heat thus absorbed is nowise distinguished from the rest, or fitted to perform any different function; it blends its action and its expansive energies with the general fluid, and merely serves to restore the equilibrium that had been disturbed by the enlarged capacity, or rather the increased attraction, of the mass with which it combines."——

"The gaseous substances are so loosely constituted, that a difference in their composition is sufficient to alter materially

terially their intimate properties. Thus, common air, on being condensed 30 times, has its capacity for heat reduced to one half; and, if suddenly compressed to 20 times its ordinary density, it will disengage so much heat as to show an elevation of temperature equal to 900 degrees by Fahrenheit's scale, and sufficient for the inflammation of most bodies. On this property is founded a pretty contrivance lately made, the stroke of a small condensing syringe being employed to set on fire a bit of tinder. An opposite effect, when air is suddenly rarefied, takes place; a certain quantity of heat being now absorbed, or an apparent cold produced.

"The increased capacity of rarefied air is the true cause of the cold which prevails in the higher regions of the atmosphere. From the unequal action of the sun's rays and the vicissitudes of day and night, a perpetual and quick circulation is maintained between the lower and the upper strata; and it is obvious, that, for each portion of air which rises from the surface, an equal and corresponding portion must also descend. But that which mounts up, acquiring an enlargement of capacity, has its temperature proportionally diminished; while the correlative mass falling down carries likewise its heat along with it, and, contracting its capacity, seems to diffuse warmth below. A stratum at any given height in the atmosphere is hence alike affected by the passage of air from below, and by the return of air from above, the former absorbing heat, and the latter evolving it. But the mean temperature at any height in the atmosphere is still on the whole permanent, and consequently those disturbing causes must be exactly balanced, or the absolute measure of heat is really the same at all elevations, suffering merely some external modification from the difference of capacity in the fluid with which it has combined. That temperature is hence inversely as the capacity of air possessing the rarity due to the given altitude. Having therefore ascertained, by some delicate experiments, the law which connects the capacity with the rarity of air, it was not very difficult to trace the gradations of cold in the higher atmosphere, and even to mark the precise limit where the reign of perpetual congelation must commence. Thus, I find that, under the equator, the boundary of the frozen region begins at the altitude of 15207 feet, in the parallel of 45° at 7671 feet, in the latitude of London at 5950, and in that of Stockholm at 3818, while towards the pole it comes to graze along the surface."

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The mode by which heat is conducted through substances differently constituted is various: in solid bodies it is by successive impressions: in fluids the mobility of the particles affects the mode of operation: the proximate portion of the medium, dilating as it becomes warm, is gently forced to recede; and being likewise rendered specifically lighter it rises to the surface, diffused in horizontal strata, the hottest particles occupying the highest part: hence heat descends in fluids very tardily and with extreme difficulty; a circumstance which accounts for the great coldness of water at the bottom of deep lakes.

"The increasing coldness of the water drawn up from considerable depths in the ocean has lately been proposed as a sure mark of the approach of soundings, if not of the land itself.

"Since water, on being heated, expands in a rapid progression, the portion of heat which it abstracts from a body immersed in it, by means of the recession and incessant change of its contiguous affected particles, must be greatly augmented in the higher temperatures. Near the freezing point, this influence becomes extremely small, and water is there scarcely a better conductor than ice; but, as it approaches to ebullition, it acquires such an increase of mobility, as to conduct heat five times faster than in its torpid state. In other liquids, the increase of temperature will occasion a similar alteration of the conducting powers, though not so marked, as their expansions deviate less from an uniform progression.

"But, through air and other gaseous fluids, the conveyance of heat is still more complex; and a close investigation of that process, by unfolding certain latent properties of matter, has led to some very unexpected and interesting results. A new principle appears to combine its influence, and the rate of dispersion, in aëriform media, is found to depend chiefly on the nature of the mere heated surface. From a polished metallic surface, heat is feebly emitted; but, from a surface of glass, or still better from one of paper, it is discharged with profusion. If two equal balls of thin bright silver, one of them entirely uncovered, and the other sheathed in a case of cambric, be filled with water slightly warmed, and then suspended in a close room, the former will lose only 11 parts of its heat in the same time that the latter will dissipate 20 parts. Of this expenditure, 10 parts from each of the balls is communicated in the ordinary way, by the slow recession of the proximate particles of air, as they come to be successively heated.

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The rest of the heat, consisting of 1 part from the naked metallic surface, and of 10 parts from the cased surface, is propagated through the same medium, but with a certain diffusive rapidity, which in a moment shoots its influence to a distance, after a mode entirely peculiar to the gaseous fluids. The very superior propellent energy of a surface of glass or paper in comparison of that of a metallic one, lies within the compass even of ordinary observation. If a glass caraffe or a pot of porcelain be filled with boiling water, on bringing towards it the palm of the hand, an agreeable warmth will be felt at the distance of an inch or two from the heated surface; but if a silver pot be heated in the same way, scarcely any heat is at all perceptible on approaching the surface, till the fingers have almost touched the metal itself.

"It is curious to inquire how such a singular diversity can arise. If the silver ball be covered with the thinnest film of gold-beater's skin, and which exceeds not the 3000th part of an inch in thickness, the power of dispersion will be augmented from 1 to 7; if another pellicle be added, there will be a further increase of this power, from 7 to 9; and so repeatedly growing, till after the application of five coats, when the propellent energy will reach its extreme limit, or the measure of 10. In this case, the metallic surface is precluded from all contact with the air, and it must, therefore, act in consequence of its mere approximation to the external boundary. We may thence infer, that air never comes into actual contact with any surface, but approaches much nearer to glass or paper than to polished metal, from which it is separated by an interval of at least the 500th part of an inch. A vitreous surface, from its closer proximity to the recipient medium, must hence impart its heat more copiously and energetically, than a surface of metal in the same condition; and the metal, to a certain extent, can act in reducing the power of the other. When a pellicle was applied, the metallic surface immediately under it repelled partially the atmospheric boundary, and reduced the darting efflux of heat from 10, which would have been thrown by the skin alone, to about 7, or only 6 more than the efficacy of the naked metal. The repelling influence of the metallic plate was sensible even under four coats, or at the distance of the 750th part of an inch from the external surface.

"By what process the several portions of heat, thus delivered to the atmosphere, shoot through the fluid mass, it seems more difficult to conceive. They are not transported

by

by the streaming of the heated air, for they suffer no derangement from the most violent agitation of their medium. The air must therefore, without changing its place, disseminate the impressions that it receives of heat, by a sort of undulatory commotion, or a series of alternating pulsations, like those by which it transmits the impulse of sound. The portion of air next the hot surface, suddenly acquiring heat from its vicinity, expands proportionally, and begins the chain of pulsations. In again contracting, this aërial shell surrenders its surplus heat to the one immediately before it, and which is now in the act of expansion; and thus the tide of heat rolls onwards, and spreads itself on all sides. These vibratory impressions are not strictly darted in radiating lines, but each successive pulse, as in the case too of sound, presses to gain an equal diffusion. Different obstructions may, therefore, cause the undulations of heat to deflect considerably from their course. Thus, if successive rings of pasteboard be fashioned into the twisted form of a cornucopia, and its wide mouth presented at some distance to the fire, a strong heat will, in spite of the gradual inflection of the tube, be accumulated at its narrow end; in the same manner probably, as waves, flowing from an open bay into a narrow harbour, now contracted and bent aside, yet without being reflected, rise into furious billows.

"But the same pulsatory system will enable the atmosphere to transmit likewise the impressions of cold. The shell of air adjacent to a frigid surface, becoming suddenly chilled, suffers a corresponding contraction, and which must excite a concatenated train of pulsations. This contraction is followed by an immediate expansion, which withdraws a portion of heat from the next succeeding shell, itself now in the act of contracting; and the tide of apparent cold, or rather of deficient heat, shoots forwards with diffusive sweep. The energy of transmission is subject, in this case also, to the same modifications from the nature of the affected surface. Thus, a goblet filled with pieces of broken ice, or still better with a frigorific mixture composed of snow and salt, will, at a moderate distance, yet excite a chilling sensation; but a silver pot, filled with a similar mixture, will not cool the hand, till it has become profusely covered with dew, and therefore now presents a non-metallic surface.

"But the same quality by which a surface propels the hot or cold pulses, equally fits it, under other circumstances, to receive their impressions. If a vitreous surface sends forth

forth its heat the most copiously, it will also, when opposed to the tide, arrest with entire efficacy the affluent wave; and if, on the other hand, a surface of metal sparingly parts with its heat, it in like manner detains only a small share of each appulse, and reflects all the rest. The power of superficial absorption and that of reflection are therefore exactly contrasted, and the one always supplies the deficiency of the other. The naked bulb of a thermometer held near a goblet full of boiling water, will mark a very sensible afflux of heat; but if it be gilt or covered with tin-foil, it will scarcely seem at all affected. For the same reason, the hand cased in a glove of burnished metal may approach the fire with impunity, since the vehement pulsations of heat are mostly driven back, or turned aside from their attack. A sheet of paper, opposed to the aërial tide, will absorb the whole impression, a pane of glass will repel about one-tenth part, while a plate of polished silver will reflect nine-tenths of the heat, detaining only the remaining tenth. But if the metallic plate be covered with a pellicle of the 3000th of an inch in thickness, out of 10 parts of heat no more than three will be reflected, the rest being now absorbed; and by applying successively other pellicles, till a coat equal to the 500th of an inch in thickness has been formed, the quantity of reflection will gradually become insensible. The power of a metallic speculum in concentrating at its focus the pulses of heat or cold is hence very striking, while the corresponding effects of a glass mirror seem to be extremely feeble.

"The very different powers of a vitreous and of a metallic surface in propagating or absorbing the pulsations of heat, are well contrasted by an experiment of the simplest and easiest kind. Let a small pane of glass about four inches square have one of its sides half covered with smooth tin-foil; or, what is more elegant, let a small square of thin mica have one side plated half over with silver leaf. On holding the partly covered surface of the glass or mica opposite and very near the fire for the space of a few seconds, and then passing the finger lightly over the posterior surface, scarcely any warmth is perceptible under the metallic sheath, but an intense degree of heat will be felt behind the naked portion of the plate. Again, reversing its position and exposing the uncovered side to the fire, an opposite though less marked effect is observed; the coat of metal will become sensibly hotter than the adjacent naked space; because the heat absorbed along the interior surface being afterwards more feebly discharged from the tin or silver leaf, is

is allowed to accumulate in that part of the screen. In this latter case, the difference of temperature produced is very nearly the double, and in the former it is no less than tenfold. But effects of the same kind, and which are alike contrasted, though inferior in degree, will be perceived, if a thin pellicle be spread over the compound surface of the glass and tinfoil, or of the mica and silver leaf, the mere proximity of the metallic surface repelling the atmosphere, and consequently enfeebling the powers of absorption and emission.

"The very singular and unexpected facts now detailed merit attention, and suggest a variety of improvements in the practical management of heat. A vessel with a bright metallic surface is the best fitted to preserve liquors either long warm, or as a conservatory to keep them cool. A silver pot will emit scarcely half as much heat as one of porcelain; and even the very slightest varnishing of gold, platina or silver, which communicates to the ware a certain metallic gloss, renders this new kind of manufacture about one-third part more retentive of heat. The addition of a covering of flannel, though indeed a slow conductor, far from checking the dissipation of heat, has directly the contrary tendency; for it presents to the atmosphere a surface of much greater propulsive energy, which it would require a thickness of not fewer than three folds of this loose substance fully to counterbalance. The cylinder of the steam-engine has lately been most advantageously sheathed with polished copper.

"The progress of cooling is yet more retarded, by surrounding the heated vessel on all sides, at the distance of near an inch, with a case of planished tin; and the addition of other cases, following at like intervals, augments continually the effect. With an obstruction of one case, the rate of refrigeration is three times slower, with two cases it is five times slower, with three cases it is seven times slower, and so forth, as expressed by the succession of the odd numbers. By multiplying the metallic cases, therefore, and disposing them like a nest at regular intervals, the innermost could be made to retain the same temperature with little variation for many hours or even days. Such an apparatus would obviously be well calculated for various culinary and domestic purposes.

"In the conveyance of heat by means of steam, the surface of the conducting tubes should have a metallic lustre. On the contrary, if it be intended by that mode to warm an apartment, they should be coated on the outside with soft

 paint,

paint, to facilitate their discharge of heat. For the same reason, metallic pots are more easily heated on the fire, after their bottoms have become tarnished or smoked. If a bright surface of metal be slightly furrowed or divided by fine flutings, it will emit heat sensibly faster, because the prominent ridges, thus brought closer to the general atmospheric boundary, will excite the pulsations with augmented energy.

"On the other hand, a plate of metal, however thin, if only burnished on each side, will form the most efficacious screen. A smooth sheet of pasteboard, gilt over both sides, would answer the same purpose. But a complete and elegant screen might be composed of two parallel sheets of China paper, placed about an inch asunder, and having their inner surfaces gilt, and their outsides sprinkled with flowers of gold and silver.

"Since, in a still atmosphere, the momentary flow of heat from any vessel, whatever this may contain, depends merely on the condition of its surface, the whole accumulated discharge, during similar descents of temperature, is evidently proportional to the time elapsed. Hence a very simple and accurate method is suggested, for ascertaining the capacity of different liquids or their specific attraction to heat. Into a glass ball, two or more inches in diameter and blown extremely thin, with a narrow short neck, and having a delicate thermometer inserted through it, the liquid to be examined, which had been previously warmed a few degrees, is carefully introduced by means of a funnel. The ball is then made to rest against the tapering points of three slender glass rods at the height of several inches above the table, and sheltered from any irregular agitation of the air of the apartment by a large receiver passed over it. The number of seconds which the thermometer now takes to sink from one given point to another, or to the middle of its distance from the limiting temperature, is noted by help of a stop-watch; and the ball being thoroughly emptied and again successively filled with other liquids, the like observations are repeated. These several intervals of time, allowing a slight correction for the matter of the shell itself and of the inserted bulb of the thermometer, will consequently express the proportional quantities of heat contained in equal bulks of the successive liquids. But their densities being already known, it is hence easy to compute their respective capacities, or the quantities of heat which equal weights of them are capable of containing. By a process grounded on the same principles, the capacity of

of a solid, when broken or reduced to a gross powder, may be determined.

"The same regulated mode of cooling will serve to detect with precision the expenditure of heat, and to discriminate its various allotment, in the different gases. For this purpose, a ball of about three inches in diameter, and formed of bright and very thin silver, is preferable; and it may be successively covered with a pellicle or with cambric, or painted with a coat of ivory-black. Not to multiply unnecessary details, it will perhaps be deemed sufficient to cite the case of hydrogen gas, which is by far the most distinguished. The portion of heat emitted in this energetic species of gas by the system of pulsations, whether from a vitreous or a metallic surface, if not exactly, is very nearly the same as in atmospheric air; but that other portion which is abstracted by the gradual recession of the nearest heated particles of the fluid, exceeds no less than four times the corresponding discharge in the ordinary medium. Why such a striking difference should arise, can hardly be conjectured. Hydrogen gas, though ten times lighter than air, yet contains, in the same volume, an equal quantity of heat; and it is fitted, by its very superior elasticity, to transmit the pulsatory impressions more than three times faster. It must, therefore, as a counterbalance, receive those impressions three times slower from the heated surface. But if such influence be confined, as it would seem most probable, to the mere boundary of the medium or its thinnest conterminous shell, the measure of heat imbibed at a given rise of temperature from the attenuated expanse, would be diminished between two and three times. This mutual compensation of effect nearly agrees with the actual result. With respect to the quadrupled increase of that portion of heat which is abstracted by the slow but continued renewal of the adjacent stratum of the fluid, we must refer it chiefly to the very great mobility of hydrogen gas, exceeding three times that of common air. If these strata were supposed to have in both cases the same thickness, they would each of them carry off the same share of heat.

"The portions of heat transmitted by pulsation through hydrogen gas, from a painted and a metallic surface, being, as before, expressed respectively by 10 to 1, the other portion, which is altogether independent of the nature of the cooling surface, and is dispersed by abduction, or the incessant retreat of the strata of the fluid as they come to be successively affected, will amount to 40. Under like circumstances, therefore, the whole expenditure of heat from

 a painted

a painted and a metallic surface, and which in atmospheric air was denoted by 20 and 11, will in hydrogen gas be represented by 50 and 41. Those opposite surfaces are thus less contrasted in a medium of hydrogen gas, their different rates of discharging heat being nearly in the proportion of 5 to 4. The silver ball cased with cambric, cools $2\frac{1}{2}$ times faster, if immersed in hydrogen gas; but when exposed naked in the same fluid, it loses its heat almost four times as fast as in common air.

"The superior mobility of hydrogen gas accelerates remarkably the dispersion of heat, by the process of abduction. But the exposing of a heated body to the action of any current of a fluid substance, will occasion a similar expenditure of heat, and which is exactly proportioned to the celerity of the stream. If a very large bulb of a thermometer be suddenly plunged into water flowing at the rate of one-third of a mile in the hour, it will be found to lose its heat twice as fast, as when immersed in the stagnant pool; and a current of two miles in the hour would, therefore, cause through the liquid a dissipation of heat no less than seven times more rapid than usual. A similar acceleration of effect is produced, by the impulse of a stream of air. With a velocity of about four miles in the hour, the superadded influence of a current equals the ordinary power of abduction. Hence the play of a breeze of eight miles an hour will double the rate of cooling from a painted, and will triple that from a metallic, surface; but a wind sweeping with a velocity of forty miles in the hour, would accelerate the cooling of the painted surface six times, and that of the metallic one no less than eleven times, thus bringing them both near an actual equality of performance. In general, the hourly velocity of wind might be computed, by multiplying eight miles into the proportional surplus effect exerted in the refrigeration of a vitreous or painted surface.

"But even in still air, if the body exposed to its action have a very considerable elevation of temperature, the progress of cooling will be sensibly quickened, by the continual ascent of the heated portions of the medium, and which form in fact a stream, varying in force according to the intensity of excitement. Supposing the excess of temperature to be 30 centesimal degrees, or 54 by Fahrenheit's scale, this gentle perpendicular flow of heated air will conjoin an influence equal to the ordinary abductive dispersion of heat, and therefore corresponding to that of a current which moves at the rate of four miles an hour. Hence, if the silver

silver ball be 90 centesimal degrees hotter than the encircling air, the effect of the vertical stream is tripled, or the aggregate expenditure, from the painted and from the naked surface, will be expressed by 50 and 41, the dissipation arising from the increased flow of the medium amounting in each of them to 30. By a singular coincidence, this proportion is precisely the same as what obtains near the equilibrium of temperature in an atmosphere of hydrogen gas. But hydrogen gas betrays in its own constitution still greater modifications. At the same elevation of 90 centesimal degrees of temperature, the combined powers of cooling which its exerts on the contrasted surfaces, are expressed by 170 and 161.—It would be fatiguing, however, to pursue this intricate analysis much further."

LXIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

May 27.—Earl Morton in the chair.—Mr. Brande, through the medium of the Society for promoting Animal Chemistry, furnished some additional observations on the use of magnesia and acids in the case of calculous diseases, where alkalies had failed or proved injurious. He related the case of a gentleman of the law, who was closely confined to business during terms, and in consequence of such sedentary habits suffered severely by calculi. The use of magnesia in this and several other cases effected a complete cure, even where the patients suffered by the passage of stones from the kidneys to the bladder, and where the use of alkalies had only aggravated their sufferings by impairing the digestive powers of the stomach. In all such cases, however, the stone or gravel consisted of uric acid and phosphate of lime, which were recognised by the red colour of the urine and its sediment. But another species of calculus was discovered by Dr. Trolleston about fifteen years ago, being a triple salt and consisting of ammoniacal magnesian phosphate of lime. No remedy has hitherto been proposed for it. This calculus is known by its whiteness, the whitish sediment, and the thin shining crystalline pellicle swimming on the urine. It is not soluble by magnesia, but yields to the muriatic, citric, and carbonic acids: the latter was found the most effectual, and least offensive to the stomach.

June 3.—The conclusion of Mr. Brande's paper was read, and the result of his observations and experiments must

must be gratifying to all calculous patients, as furnishing them with some well-grounded hopes of a speedy and certain remedy. The use of muriatic and carbonic acids continued for some months effected permanent cures, even where the patients had been previously cut for the stone. The Society then adjourned over the Whitsuntide festival till

June 17.—The right hon. Earl of Morton in the chair. A letter from Mr. Exley of Bristol to the President, and by him communicated to the Society, was read, on the Theories of Electricity and the difficulties attached to them. The author prefers the Franklinian theory to all others, and endeavoured to explain those parts of it which are least compatible with the known facts. He defined the terms plus and minus or positive and negative electricity somewhat in the usual manner, assumed that attraction and repulsion depend on the similarity of electric conditions, and showed why non-electric are good conductors, and *vice versâ.* He purposes pursuing the inquiry.

June 24. Part of a paper by Sir E. Home was read, containing additional remarks to a former paper on the anatomy of the Squalus maximus. The most remarkable fact was the existence of a small fin between the anus and the tail, which was overlooked in his preceding observations, and which contributed to lead naturalists into serious errors. Sir E. entered into some details, which were illustrated by drawings, on the general structure of this squalus, which was thirty feet long, and that of a dog-fish of only three feet. The inquiry respecting the supply of air to fishes was also touched on, and Sir E. made some experiments on the effect of pressure of water at a considerable depth, and particularly in a well 680 feet below the surface of the Thames, belonging to Mr. Coutts. The object was to prove that the atmospheric air in water at that depth was the same as in water near the surface. Sir E. made some remarks accompanied with speculations on the expanding and contracting powers of the vessels in the gills of sharks, which enabled them to sustain the pressure of water at 150 fathoms below the surface.

GEOLOGICAL SOCIETY.

June 4.—The President in the chair.

The Duke of Devonshire;
John Whishaw, Esq. of New Square, Lincoln's Inn;
Henry Drummond, Esq.;

Charles

Charles Price, M.D. Fellow of Wadham College, Oxford;
William Lowndes, Esq. of Somerset Place;
Viscount Kirkwall, M.P.;
Alexander Sutherland, M.D. of Great George Street, Westminster;
George Wilbraham, Esq. of Upper Seymour Street, Portman Square;

were severally elected members of the Society.

An account of the Isle of Man, by S. F. Berger, M.D. M.G.S. was read.

The length of the Isle of Man from NE to SW exceeds thirty English miles, and its breadth varies from eight to fifteen miles. About five miles from the northern extremity a mountainous tract commences running parallel to the eastern coast of the island, and also forming the small detached island called the Calf of Man situated at the southern extremity of the larger one. This belt or chain of high land is divided by three transverse valleys, of which two are situated in the larger island, and the third forms the strait that separates the one island from the other. The highest mountains are situated in the northern division, the most elevated of which, called Sneifeldt, is 2000 feet above the level of the sea.

The rocks of which this country is composed belong chiefly to the transition class of Werner. Small grained granite occurs only in one or two places, and at an elevation of not more than three or four hundred feet above the sea. Gneiss and mica slate appear to be entirely wanting, as also are the oldest members of the clay slate formation. The newer portion of the clay slate formation occupies the most elevated parts of the island, where it appears under the form of horn slate, roofing slate.

From these rocks the passage to the transition class takes place by insensible degrees: and of this the oldest member that presents itself is gray wacke. The tract occupied by this latter rock is for the most part less elevated than that where the clay slate makes its appearance and incloses it. The beds dip south more or less to the east, and this inclination varies from vertical to about 35°. In this formation occurs gray wacke, gray wacke slate, and granular quartz, slightly micaceous; in none of which rocks are any organic remains to be perceived.

The preceding formation is covered by a deposit of limestone less elevated above the sea than the gray wacke, and at an inclination approaching nearer to horizontal. It consists of beds of shell limestone, resembling that of Kilkenny,

kenny, and of Westmoreland, Cumberland, and Durham, together with magnesian limestone, sometimes in separate beds, and often in distinct patches, inclosed within the other. This magnesian limestone, except in a single instance, appeared destitute of organic remains, but in some places incloses roundish nodules of glassy quartz.

In one or two places the limestone is covered by an unstratified mass of transition amygdaloid; the base of which is a greenish wacke, containing nodules of lamellar calcareous spar invested by a thin coating of iron pyrites.

Of the flœtz or secondary rocks the only one that occurs is the oldest sandstone, some of the beds of which are so coarse-grained as to merit the name of conglomerate, in which case it consists chiefly of fragments of quartz, with a few scraps of decayed slate, and a little iron pyrites. The colour of the sandstone is red, or grayish white; it is more or less slaty, according to the proportion of mica that it contains: it lies unconformably over the gray wacke, and dips NW at an angle varying from 35° to 15°.

On the sea shore, and on the slopes of several of the mountains, are loose blocks, in great abundance, of granite, of mica slate, and of porphyry.

The only mines in the island are at Loxey, at Foxdale, and at Brada head: at present however they are all abandoned. The ore is galena mixed with pyrites, and with the carbonates of lead and of copper. The rock through which the veins run is gray wacke: but at Foxdale they have been followed into the subjacent granite.

The paper is terminated by two tables. Of these the first is a register of the temperature of several springs ascertained during the month of June 1811. From this it appears that the mean temperature of the island 49°.99, exceeding that of Edinburgh by about 2°.2, and inferior to that of London by about 1°.

The second table contains the elevation of 78 different spots in the island, deduced from barometrical observations. Of these there are twenty-one the height of which is between 1000 and 2000 feet above the level of the sea.

June 18.

Sir Henry Englefield, Bart., Vice President, in the chair.

The Rev. Edward Hony, Fellow of Exeter College, Oxford;
The Rev. George Barnes, Fellow of Exeter College, Oxford;
John Hanson, Esq. of Bloomsbury Square;
John Forster Barham, Esq. M.P. of Queen Anne Street;
Thomas Bigges, Esq. of Brompton;
Samuel Turner, Esq. of Nottingham Place;
were severally elected members of the Society.

A letter from James Curry, M.D. M.G.S. was read.

In this letter Dr. C. describes a remarkably large specimen of nodular agate (exhibited before the Society) which he conceives to point out a natural connection between agate and the plasma of the ancients.

The reading of Mr. Webster's paper "On the Freshwater Formations of the Isle of Wight, with some Observations on the Strata lying above the Chalk in England," was begun.

The observations in this paper were in part suggested by the recently published memoir of MM. Cuvier and Brongniart, concerning the strata in the vicinity of Paris, in which they have described two marine and two fresh-water formations alternating with each other, the whole lying above the chalk, which latter rock has hitherto been very generally considered as one of the most recent deposits.

It is to Sir Henry Englefield that we are indebted for the first observation of highly-inclined strata of chalk in the Isle of Wight.

A circumstance so material for the theory of the formation, or of the revolutions undergone by the more recent strata of the earth, demanded a leisurely and careful survey, which was intrusted by Sir H. Englefield to the well-known accuracy of Mr. Webster. The present paper is the result of this inquiry.

An elevated ridge of hills runs through the Isle of Wight, in a direction nearly E and W, from Culver Cliff to the Needles. These hills are composed of strata sometimes nearly vertical, but generally forming an angle with the horizon of from 60° to 80°, dipping northward. The strata consist of the upper and lower beds of chalk, that is the chalk with and without flints, covering the chalk marl; and these again are underlaid by calcareous sandstone with subordinate beds of chert and limestone, clay and carbonized wood. To the north of these strata occur, at Alum Bay, other vertical beds of sand and clay, one of which corresponds in its fossils and other characters with the blue clay containing aptaria, usually known by the name of the London clay.

The whole series of vertical beds exhibits no marks of partial disturbance: but it is evident, from the occurrence of these very same beds in other parts of the country in a nearly horizontal position, and from the impossibility of some of them (consisting of loose sand with water-worn nodules of flint) being deposited in the vertical position in which they are at present, that the whole mass must have been

been bodily raised or depressed by some unknown force applied to them, subsequently to the formation of the bed of London clay.

If a line in the direction of the central ridge of the Isle of Wight, be extended westwards into Dorsetshire, it will be found to coincide nearly with the direction of a ridge running from Handfast Point to Lulworth, and with that already described; and which therefore may be considered as a continuation of this former.

The nearest tract of chalk to the north of this ridge is the South Downs, the strata of which, together with their superimposed beds up to the London clay, dip gently to the south. Hence the space between may be considered as a great basin or hollow, occasioned probably by the rupture and subsidence of strata originally horizontal.

Within this basin at its southern edge, that is on the northern coast of the Isle of Wight, occurs a large mass of horizontal strata in many parts visibly resting on the edges of the elevated strata above mentioned, and therefore belonging to a period subsequent to that in which the formation of the basin took place. This horizontal deposit differs in its geological situation, in its mineralogical characters, and in the fossils which it contains, from any others that have hitherto been discovered in England; but remarkably corresponds in many of its members with the beds found in the basin of Paris, and recently described by MM. Cuvier and Brongniart; authenticated specimens of which, sent by the latter of these gentlemen to the Count de Bournon, have been by him deposited in the cabinet of the Geological Society.

These beds as they appear in the Isle of Wight constitute four formations: the first of which is the lowest fresh-water formation; the second is the upper marine formation; the third is the upper fresh-water formation; and the fourth or superficial is an alluvial bed.

The particulars of these are described in the subsequent part of Mr. Webster's paper, which has not yet been read before the Society.

PHILOSOPHICAL SOCIETY OF LONDON.

[Continued from p. 308.]

The following curious relation was mentioned by Dr. Lettsom, as occurring to an old and esteemed school-fellow. His propensity to spirits had become so irresistible, that when he had been debarred from the more grateful flavour of gin, rum, and brandy, he had purchased privately the nauseous

nauseous substitute of the volatile tincture of valerian, and the acrid stimulus of the tincture of gum guaiacum, to incite the insidious pleasure of intoxication. One night in this state he rolled out of his bed, which was approximate to a fire, the flames of which extended to his saturated body, and reduced it to a cinder, without materially injuring the bed furniture. (A case not less extraordinary is to be found in the lxivth volume of the Philosophical Transactions for 1774, by Dr. Wilmer of Coventry).

From these curious occurrences Dr. Lettsom passed on to the most infuriate state of madness from intoxication, which is excited by the use of bang combined with opium, and producing that fatal practice of running a muck, a practice that has prevailed time immemorial in Batavia, on the Malabar and Coromandel coasts, and throughout most parts of India, wherever the Malays have extended their settlements. Addicted to gaming, they indulge this passion to the greatest extent, until they have lost every thing they possessed, even to their wives and children; and thus lost to every possession, to drown misery, they are impelled to smoke a plant called bang with opium, till they are excited by intoxication to the highest degree of fury and madness. In this infuriate state they rush into the street, armed with a poisoned creuse or dagger, and kill every man, woman and child they encounter, till they themselves are killed or taken by the populace.

The degrees of mischief arising from diluted liquors, less active in their effects, but eventually not less painful or dangerous, fell next under the lecturer's consideration; and after relating a few instances of misery and destruction from the inordinate use of fermented liquors, he proceeded to histories of morbid effects arising therefrom. The lecturer concluded by observing, that "painful as it has been to present scenes of human infelicity, often incurred by persons of social and amiable dispositions, with hearts disposed to conviviality, and with minds formed for friendly intercourse, who have mistaken the means of rational enjoyment, by the influence of vivid imagination and of unaffected friendship, for noisy familiarity; I now cheerfully conclude, with earnestly impressing, that true happiness depends upon the œconomy and just estimate of enjoyment. It is the abuse, not the use, of wine I would deprecate. Far be it from me to proscribe such conviviality. It expands cordiality; it knits together friendships; it enlarges the social affections; it opens the mind; it dissipates care; it animates pleasurable sensations, and urges to promote

mote them in others; it opens the avenues of charity, and in being blessed it seeks to bless others. Without social, convivial, and rational intercourse, no liberal mind would court existence: but in these unaffected enjoyments, let us guard our best passions, and overlook the ebullitions of joy in our friends, and the human imbecilities sometimes flowing from innocence, mirth, and confidence, and which should never transpire or extend beyond the convivial and enlivening table.

> ———— When you smooth
> The brow of care, indulge your festive vein
> In cups by well inform'd experience found
> The least your bane; and only with your friends:
> These are sweet follies; frailties to be seen
> By friends alone, and men of generous minds."

KIRWANIAN SOCIETY OF DUBLIN.

The Society having concluded the session of the present year we shall lay before our readers an abstract of an address delivered by M. le Chevalier MacCarthy "On the Rise and Progress of Learning, and the beneficial Influence of Literary and Scientific Societies."

The address being of considerable length, and composed of a number of particulars, we shall select only the leading topics, and endeavour as much as possible to maintain the concatenation; although, from the nature of the matter, an air of hurry or even of confusion is nearly unavoidable.

Towards the commencement of the session "An Historical Review of the Scientific Literary and Philosophical Works of the late Richard Kirwan, Esq., LL.D. F.R.S., &c." was read by J. O'Reardon, Esq., M.D. Of this also we would have given an abstract; but that it was impossible to condense the subject matter, with the commentaries of the author, into a compass sufficiently small for this place, without rendering the whole uninteresting and spiritless.

The Right Honourable the President having left the chair, M. le Chevalier MacCarthy took his seat, and delivered his address, of which the following is an abstract:

During a series of ages which were to the natural sciences what the seventh, twelfth, and thirteenth centuries were to literature, we are compelled to wade through the impenetrable shades of a long tedious night. In vain do a few feeble stars occasionally diffuse a transient illumination on our way; every step is delusion, error, uncertainty, or conjecture.

jecture. When at length in the revolution of time, we emerge from this profound obscurity, we shall be cheered by the dawn of the sun of knowledge shedding around his genial rays, and dissolving those phantoms, creatures of imagination, which in the uncertainty of darkness had assumed the appearance of reality. In this important æra we find vanishing like dreams in the morning, those antiquated and distorted systems which rested upon no other foundation than doubtful or mistaken facts, the abortive offsprings of a wild and luxuriant imagination. Such great effects have been produced by literary associations; and we must be convinced that their introduction was the first great and decisive step towards the increase and diffusion of the light of reason.

The mania of system-framing has been in ancient times productive of the worst consequences. Systems that were maintained for ages by the most celebrated characters, now appear contemptible. In what light do we now look upon the "transmigration" of Pythagoras, the "hooked atoms" of Democritus, the cosmogony of Ptolomæus, the more modern whirlwind of Descartes, and the theory of Leibnitz? Systems should be adopted with caution; they should be employed as helps, but must never be mistaken for science.

It was not for want of understanding, talent, or application, that the ancients were thus deficient in natural sciences; this mania was a principal cause: but there were others. Each individual obstinately maintained his own fanciful conceptions; the self-security of enthusiasm was incompatible with the modest caution of deliberate investigation: each was insulated with regard to philosophical communication; the means which we now possess in the art of printing, of diffusing what is known, was wanted, and there was no common repository for fact and observation, from which alone truth could be deduced. Thus, without detracting any thing from the merits of the ancients, we can account satisfactorily for their deficiencies.

Yet so admirable is the disposition of human affairs, that even these deficiencies were productive of the most salutary effects. In the infant state of society, the first efforts of men were to supply the necessities and comforts of life; next, to lay the foundation of laws, morality, and religion. At a more advanced period, the study of human nature, of poetry, oratory, cosmogony, and metaphysics, was successfully cultivated. Natural appearances no less striking in their grandeur than perplexing in their variety, were witnessed

although not understood. Nature proclaimed her wonders to an astonished world; Terror enforced the belief of a supernatural agent; men sunk with reverential awe before that invisible power which they could neither discover nor avoid, and their very ignorance compelled them to acknowledge the great, the incomprehensible Creator of the world.

Not so had the ancients been in possession of our explanations of natural phænomena: they would have raised their thoughts no higher than effects, without reflecting on the necessity of an ultimate cause: in fine, they would have sunk into the careless apathy of deism, perhaps of atheism.

In these early ages we find even the philosophical poets endeavouring to account for the formation and continuance of the world without allowing the interference of a Divine power. By their supposed success we find them in a great degree divested of those terrors about futurity which otherwise prove so powerful a check upon human passions. This appears evidently from the following passage in the "De Rerum Natura" of Lucretius:

> Nam simul ac ratio tua cœpit vociferari
> Naturam rerum haud Divinâ mente coortam,
> Diffugiunt animi terrores, &c.

Even the sweet Mantuan muse breathes the same spirit in that beautiful apostrophe near the conclusion of the second Georgic:

> Felix, qui potuit rerum cognoscere causas,
> Atque metus omnes et inexorabile fatum
> Subjecit pedibus, strepitumque Acherontis avari!

Notwithstanding the deficiencies of the ancients, and our necessary advantages over them, we should not be inflated with a consciousness of superiority: we should survey their useful labours with gratitude and respect. If we have surpassed them, they traced out our course, and we enjoy lights which never shone upon them. What they for ages sought with exemplary but unavailing perseverance, we possess without a search, being presented with these sublime truths by no less a promulgator than the Divine Author of all knowledge. They are perhaps superior to us in constancy, application, and labour: and as to their deficiencies, they should rather be attributed to want of necessary means, than to any avoidable insufficiency. With what rapture and amazement would aftertimes have looked upon Aristotle, Theophrastus, Pliny, or even Dioscorides, had they possessed our stock of experience and observation! Those classes of science which depend upon reasoning, were

were cultivated with a well known success; but those which depended upon experience were not attainable. Here then is one of the many advantages of the moderns; here the use, nay the necessity, of those associations where men of learning accumulate an invaluable store of knowledge, of the materials from which the most exquisite systems are afterwards to be constructed.

The origin of these associations is not to be placed at that remote epoch in which Ptolemy Soter or Theodosius the younger founded literary institutions; not even when Charlemagne had established one, nor for long succeeding ages. Feudal tyranny was daily extending its dominion, and strengthening its power, under the successors to the vast empire but not to the genius of Charlemagne. Ignorance was then the prerogative of nobility; learning mouldered in the recesses of monasteries; even imagination, that everlasting source of exquisite delight, was paralysed in the general torpor. In such a manner were passed six long centuries: little else had been cultivated than the art of disputation, or of perplexing every question by an interminable controversy.

But from this period opens an illustrious æra; the mists of ignorance begin to dissipate, and the cheering dawn prepares us for the splendour of the approaching day. The art of printing produced a new appearance on every thing, and the long neglected works of great men began to be disseminated. The venerable shades of the illustrious writers of antiquity seemed to rise from their ancient monuments, and to be revivified for ever. From this happy period a long line of great names may be enumerated; their labours have perpetuated their glory, and they will ever be remembered with gratitude and veneration. It is too true that in these ages the opinions of great men were sometimes wild and extravagant, although ingenious, bold, nay sublime. Of this an extraordinary instance occurs in the philosophical romances of Descartes, which will ever be remembered with interest and amazement. This great man overthrew the chimæras of the Peripatetics, even by means of other chimæras The two phantoms fought, they both fell, and after their destruction Reason reigned triumphant.

At length the period arrives in which literary associations begin to be formed; the memorable Academy at Rome, founded in 1603, was the first: amongst its members are found Galileo and Columna. It died with its founder in 1630, and in 1650 was succeeded by the Academy Naturæ Curiosorum.

In 1654 the renowned Academia del Cimento, the first truly experimental society, made its appearance: it enumerates the celebrated names of Galileo, Torricelli, Aggiunti, and Viviani. In this academy first originated the custom of publishing Transactions.

During the gloomy administration of Cromwell was formed the embryo of the since celebrated Royal Society of London. Its original members were Boyle, Evelyn, Hook, Needham, Willoughby, Ray, Lester, and Grew. Of the subsequent members much might be said, had not their fame rendered it unnecessary: to eulogize, were but to name them. There was one, who, had he been the only philosopher amongst all, would have perpetuated the fame of the Society,—the illustrious, the immortal Newton, not less the lover than the beloved favourite of Nature.

In 1666 was established the famous Academy of Sciences at Paris. Amongst its early members, we find the names of Dominic Cassini and Huygens.

The splendid reputation, indefatigable labours, and incalculable services of these three academies, in England, France and Italy, excited a spirit of emulation in every part. Every monarch was ambitious of being a protector, and every man of talent an associate, of some learned society. The number of academies that sprung up at once was amazing; no less than 550 have been enumerated in Italy alone.

In 1700 an Academy of Sciences was founded at Berlin, by Frederick I. of which Leibnitz was the chief promoter. The celebrated Maupertuis was afterwards appointed its president by Frederick III.

Peter the Great with the assistance of Leibnitz and Wolfe first projected the plan of the Imperial Academy of Sciences at Petersburg, which was established in 1725 by the Czarina Catherine I.

About 1739 Linnæus with a few men of letters formed a private society at Stockholm, which was in 1741 incorporated by the king under the title of "Academy of Sciences," and of this Linnæus was appointed president.

At length, after some unsuccessful attempts in Dublin, a number of gentlemen began to hold meetings and to read essays; and soon enlarging their plan, they were in 1786 incorporated under the name of the Royal Irish Academy, which unites the advancement of science with the history of mankind and with polite literature.

A number of societies have of late years sprung up, the latest of which is the Kirwanian Society of Dublin. The original

original members, anxious to cooperate with other institutions in promoting the cultivation of natural sciences, determined at the commencement to confine their attention to those objects exclusively, and to interfere as little as possible with the more extended views of those already established in Ireland. They resolved also to enroll themselves under the name of some philosopher who had distinguished himself in those branches of knowledge which they intended to profess. They naturally therefore turned their attention to the late celebrated and venerable Kirwan: they were anxious to do honour to the name without obliging themselves to the adoption of peculiar opinions.

[Here the Chevalier took a retrospect of the progress and labours of the Kirwanian Society; but as we have formerly given abstracts of their proceedings, they need not in this place be repeated.]

It is unnecessary to enlarge upon the invaluable benefits resulting from the introduction of academies: the period of their establishment may indeed well be called the golden age of literature. At that happy period a brilliant constellation of superior minds rose together, which illuminated the horizon of Europe with its lustre. France, England, Sweden, Germany and Ireland, may be proud of their literary career during the last century: much has been done for the benefit of society, and it ought to be the ambition of the new century before us to do as much for posterity as our ancestors have done for us. The ambition of that man, observes Lord Bacon, who attempts to establish or enlarge the dominion of the human species over the universality of things, is unquestionably more excellent and more exalted than any other: because the empire of man over things, has for its only base the sciences and arts, and it is only by obeying nature that we can learn to command her.

LXX. *Intelligence.*

Meteorological Observations made at Cambridge, from May 12 *to* 29, 1813.

May 12.—CLOUDY morning; warm day, with a hard shower in the evening; after which mountainous *cumulostrati* with *cirrostrati* transfixing their summit, dense sheets spread in a higher region, and a loose kind of *cirrocumuli*, were followed by a clear night. Thermometer midday 69°. Midnight 52°.

May 13.—Warm day; fine morning, with sun and low mountainous

mountainous or mist-like clouds, occasionally looking like the materials for more rain. Therm. at 10 in the morning, in the sun, 90°. In the shade as usual at 6 P.M. it was 62°*. Similar large *cumuli* through the day with *cirrus* aloft; gentle *nimbi* in the evening. Therm. at 11 P.M. 56°. The mist at sunset this evening was deep yellow.

May 14.—Fair showers at intervals; the usual attendants on showery weather prevailed. Therm. at 3 P.M. 66°, at 11 P.M. 55°. Wind SW. There was a double rainbow about 7 in the evening.

May 15.—Much cloud in the morning with strong wind; showers and sun at times through the day. Therm. at 3 P.M. 68°, 11 P.M. 55°. Wind westerly.

May 16.—Cloudy early, showery afternoon, with clear intervals, in which scud, *cumulostratus*, &c. appeared as usual. Among others a very long band of *cirrus* not very high up. The rainbow fairly appeared in the *nimbi*. Thermometer 3 P.M. 64°, 11 P.M. 51°. Wind SW.

May 17.—Cloudy morning, showers through the day, and some with distant thunder; in the evening the higher masses of cloud broke out into flimsy *cirrocumuli; cirrus* also visible till late; clouded again all over by midnight. Thermometer at 3 P.M. 64°; after a shower it fell rapidly to 59; at midnight 51°. Wind varying somewhat, but westerly.

May 18.—Clouded with some rain early, fair afternoon with light showers. Wind NW. and variable. Therm. at 3 P.M. 60°, at 11 P.M. 51°.

May 19.—Clouds and sun with slight showers in the morning; in the evening there was a sort of clearness, and peculiar look of the *cirrocumuli*, which I have noticed to precede fair wholesome weather. The night was clear. Therm. at 3 P.M. 63°, at midnight 51°.

May 20.—Rain in the morning; showers through the day, with vapour, clouds, &c. In the evening I observed curved lines of *cirrostratus;* much *cirrus* was also seen late, night clear. Thermometer at 11 P.M. 47°.

May 21.—Still showery; much *cumulus* and *cirrus* seen early; in the intervals of the showers confused and flimsy *cirri* and *cirrocumuli* above, rocky *cumuli* and scud below; cool air; the thermometer at 3 P.M. 58°, at 6 P.M. 54°, 11 P.M. 44°. Wind SW. and variable. I observed a fine rainbow about 7 P.M. There was also a faint appearance of a larger concentric arc. Later in the evening I observed,

* The thermometer hangs on the outside of a window: it is always registered as in the shade, unless otherwise expressed. The reader must regard the time of day at which the observation is made, or there will appear a greater disparity in the heat than really exists, or is expressed.

among

among very numerous and dissimilar appearances of the clouds, pendulous lobes as it were hanging from a sort of dense sheet, which formed a sort of depending *cirrocumulus;* afterwards a deep red was conspicuous in the cloud opposite to the set sun, as well as red in the western haze.

May 22.—Fair early; showers came on in the day, and much cloud; evening fair, except a mistiness. Therm. 11 P.M. 48°.

May 23.—Clouds in the morning with sunny intervals; they increased, and eventually small rain succeeded; towards evening the wind fell and it got warmer. Therm. at 11, 54°.

May 24.—Still showery weather, with strong west wind. Therm. at 3 P.M. 62°, at 11 P.M. 50°.

May 25.—Still showery with westerly gales, but more clouds today than rain. Therm. 63° in the middle of the day, at 11 P.M. 55°.

May 26.—Some showers during the day, various appearances of the *cumulostratus*, and in the intervals towards evening heavy *nimbi* appeared about and a rainbow *. Some *cumulostrati* in the west refracted a rich sort of mahogany colour. Orange was the precise colour of the haze. Therm. at 3, 62°, and 11 P.M. 44°. Wind westerly today.

May 27.—Fine morning, much cloud through the day; fair evening; the colour of the haze red; fading upwards as usual. Therm. at 3, 64°; at 11, 52°. Wind westerly.

May 28.—Fair day, with large masses of *cumulus;* towards evening very clear and dry, but *cirrus* in abundance variously figured about and coloured by setting sun; also some *cirrocumulus* and *cirrostratus* in mottled beds had a pretty appearance. Colour of the haze in the west reddish. Therm. 4 P.M. 62°; 11, 51°. The owls hooted more than usual; a frequent sign of fine weather, as noticed by Virgil.

May 29.—Fair hot day, very clear in the morning; during the day *cumuli* and *cirri* appeared, the latter were purple in the west after sunset; the thermometer at 7 P.M. 70°, at 11 night, 60°.

Corpus Christi College, Cambridge, June 12, 1813. THOMAS FORSTER.

* The arc was partly seen in the blue sky, a circumstance which seems to show that the particles of the nimbiform cloud may be very thinly diffused, so as hardly to alter the appearance of the sky, and yet be sufficient to produce the rainbow. This circumstance, when considered in reference to the incapability of other clouds to show the iris, may be regarded as affording collateral evidence of the peculiar change which takes place in clouds to become *nimbi.*

[To be continued.]

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For June 1813.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
May 26	51	56	50	29·72	25	Storms of hail with thunder.
27	50	60	54	30 09	39	Fair
28	54	65	54	·10	59	Fair
29	57	74	51	29·90	62	Fair
30	68	71	60	30·06	42	Fair
31	64	74	64	·07	62	Fair
June 1	66	78	67	·05	70	Fair
2	63	74	66	29·95	54	Fair
3	66	72	57	30·10	50	Fair
4	56	67	57	·20	52	Fair
5	57	58	51	29·95	32	Cloudy
6	53	55	54	·80	27	Cloudy
7	52	68	55	·80	65	Fair
8	55	68	56	·75	71	Fair
9	57	69	59	·54	36	Showery
10	58	69	57	·69	61	Fair
11	56	68	56	·70	56	Fair
12	56	68	57	·82	64	Fair
13	60	64	56	30·05	47	Fair
14	57	65	57	29·82	36	Showery
15	58	65	52	·77	60	Showery
16	55	66	51	·97	74	Fair
17	51	56	50	30·03	58	Cloudy
18	50	60	49	·07	62	Fair
19	50	54	51	·10	46	Cloudy
20	51	59	52	·12	45	Cloudy
21	54	61	52	·16	51	Fair
22	54	56	47	·18	48	Cloudy
23	50	61	58	·16	46	Fair
24	57	60	50	·19	49	Cloudy
25	55	65	53	·20	52	Fair

N.B. The Barometer's height is taken at one o'clock.

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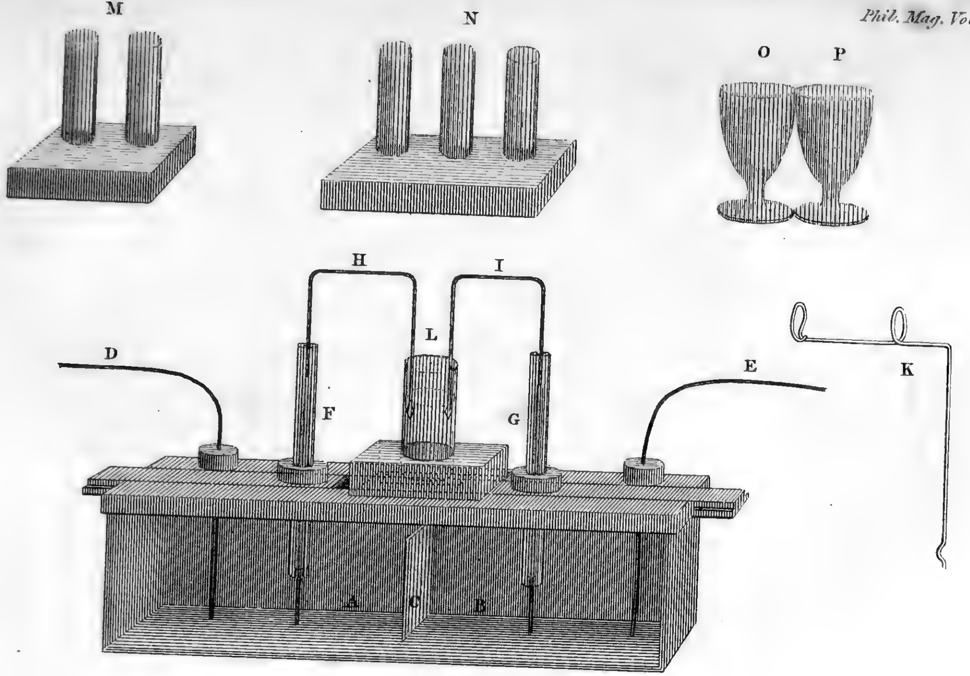
ERRATA.

Page 151, line 18, *for* credited *read* debited; ibid. line 27, *for* proportions *read* proprietors; page 234, line 17, *for* oxymuriate *read* OXYMURIATIC; page 321, line 9 from bottom, *for* and their forces *read* and these forces; page 322, line 1, *for* 10 *read* 5; page 322, line 12, *for* beams *read* beam; page 331, line 3, *for* filled *read* fitted; page 387, line 2, *for* having been *read* be.

END OF THE FORTY-FIRST VOLUME.

Printed by Richard Taylor and Co., Shoe Lane, London.



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S. Porter sc.

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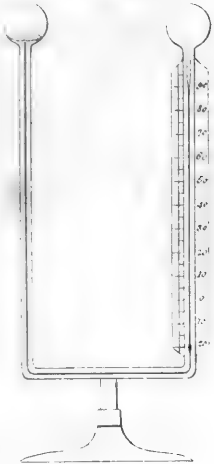

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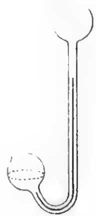

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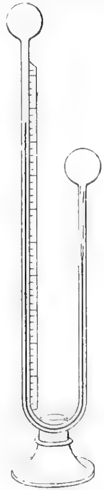

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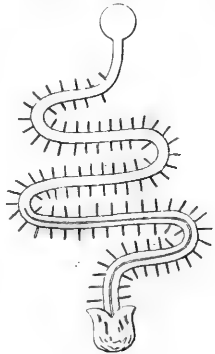

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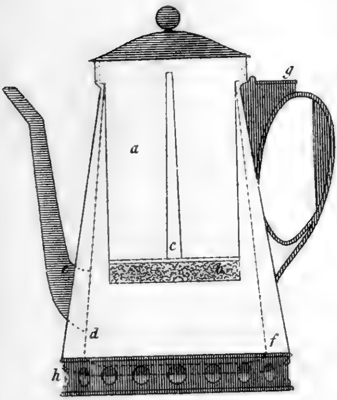

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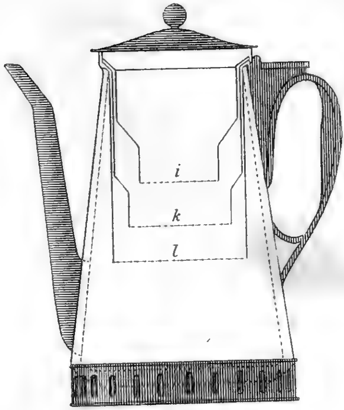

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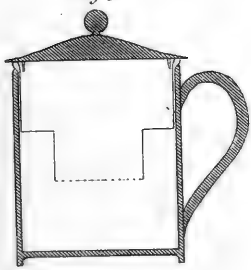

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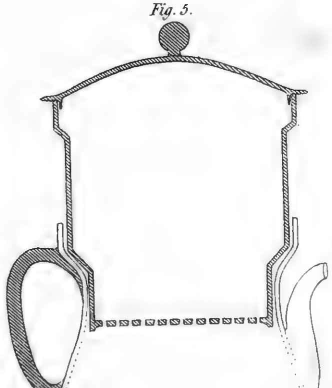

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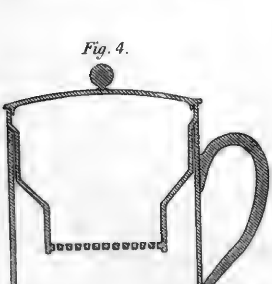


Count Rumford's
Improved Coffee Pots.

S Porter s.

Dr. Wollatson's Periscopic Camera Obscura & Microscope.

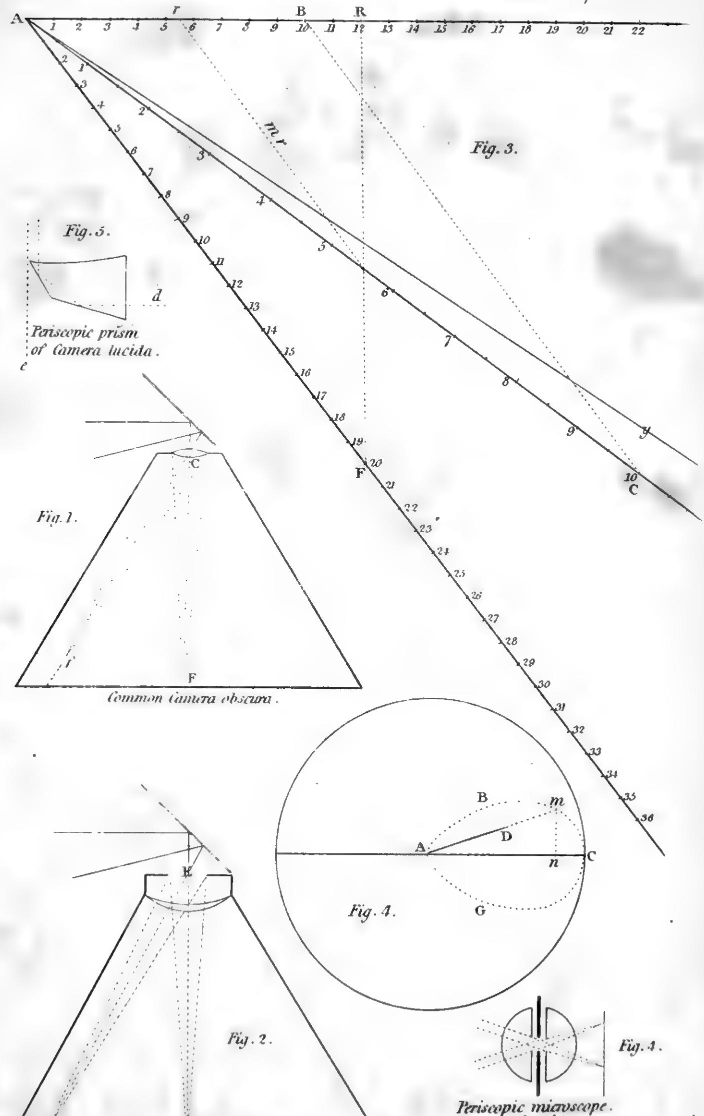


Fig. 3.

Fig. 5.

Periscopic prism of Camera lucida.

Fig. 1.

Common Camera obscura.

Fig. 4.

Fig. 2.

Periscopic Camera obscura.

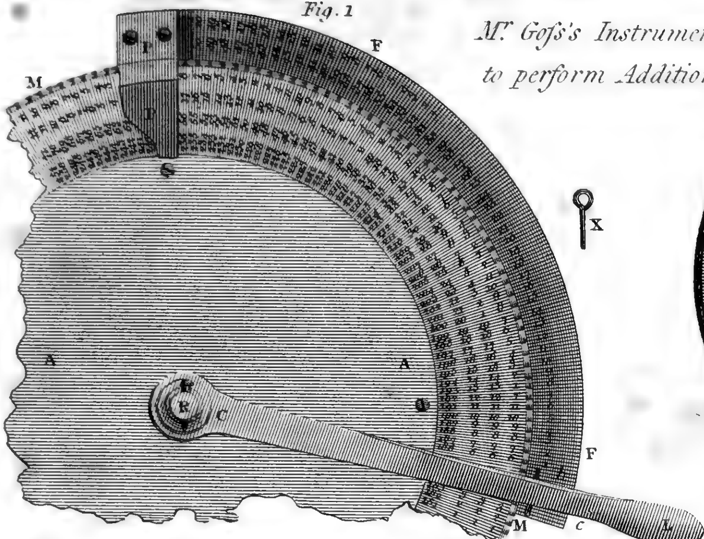
Fig. 4.

Periscopic microscope. (enlarged.)

J. Porter sc.

Mr. Goss's Instrument to perform Addition.

Fig. 1



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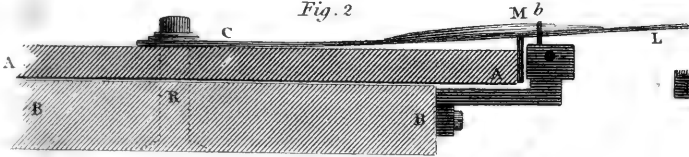

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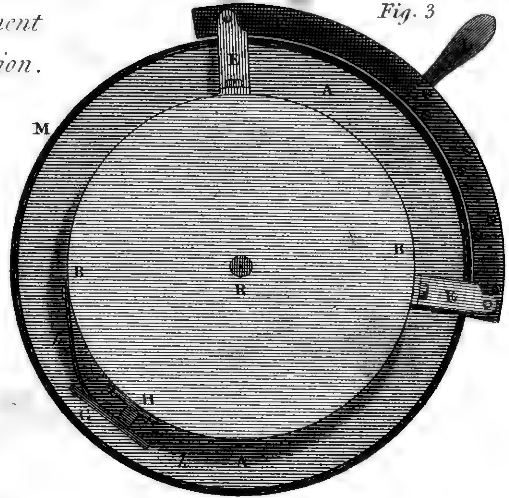

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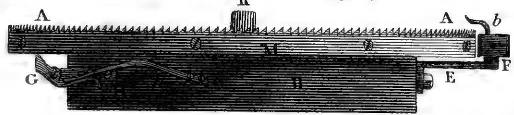

S. Porter sculp

Mr. J. Davis's temporary Scaffold for Painters, &c.

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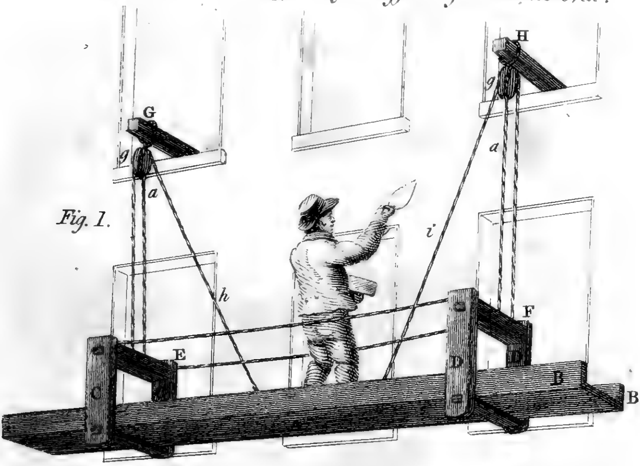
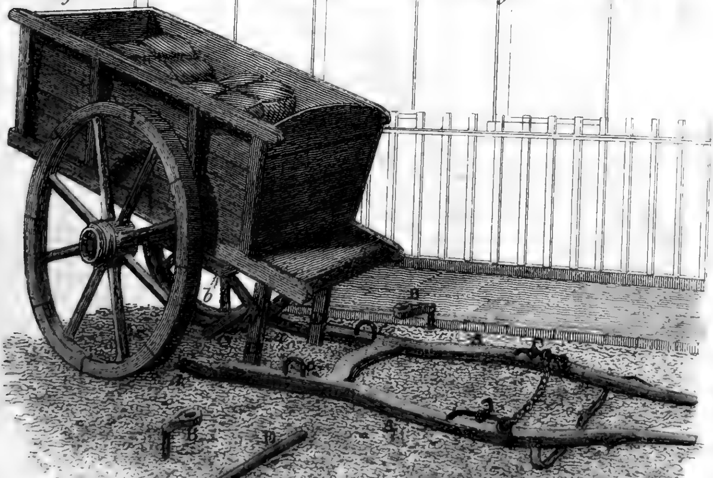
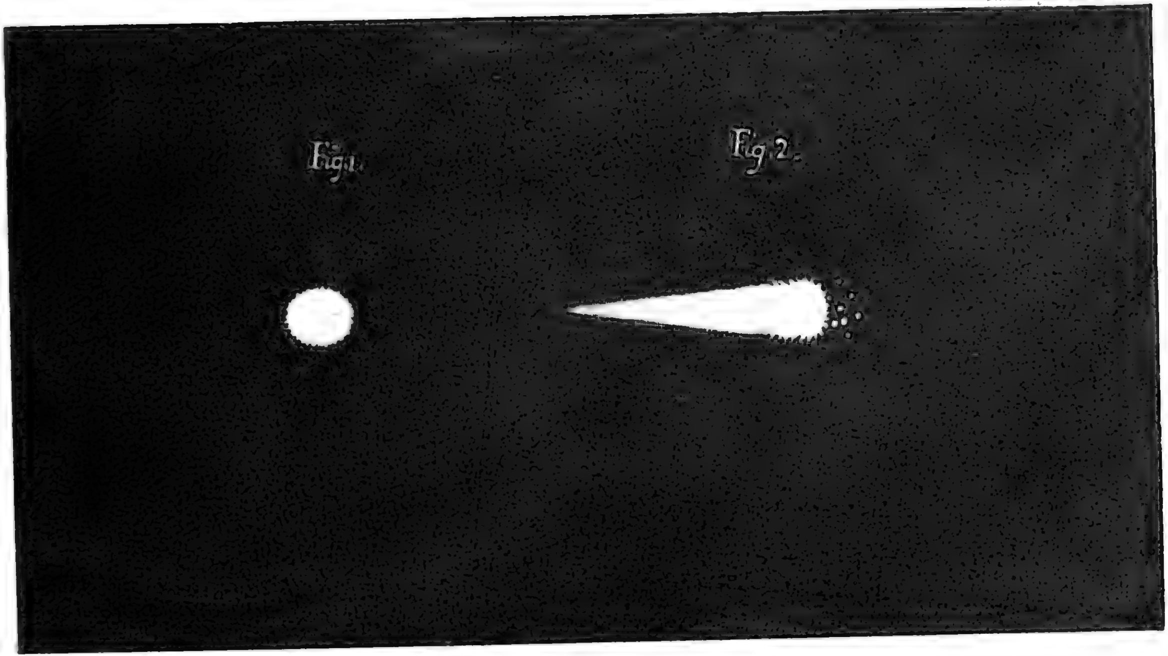


Mr. Martin's method of relieving a horse from a cart, when fallen down in its shafts.

Fig. 2.



S. Porter sculp.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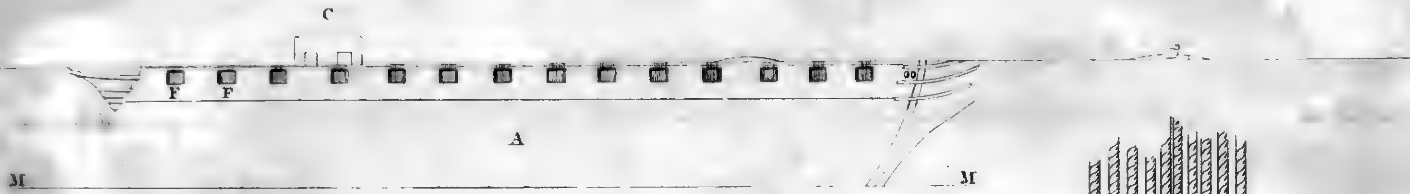
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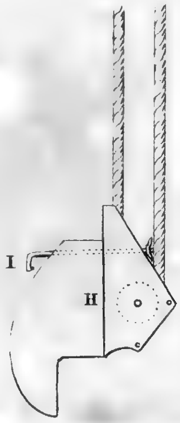
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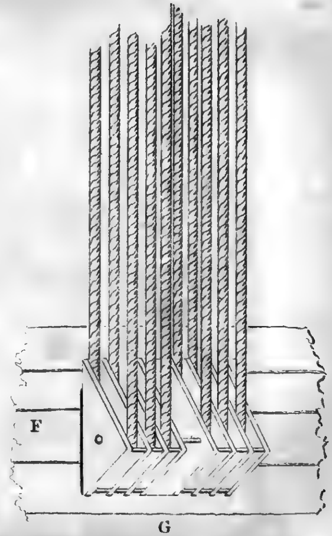
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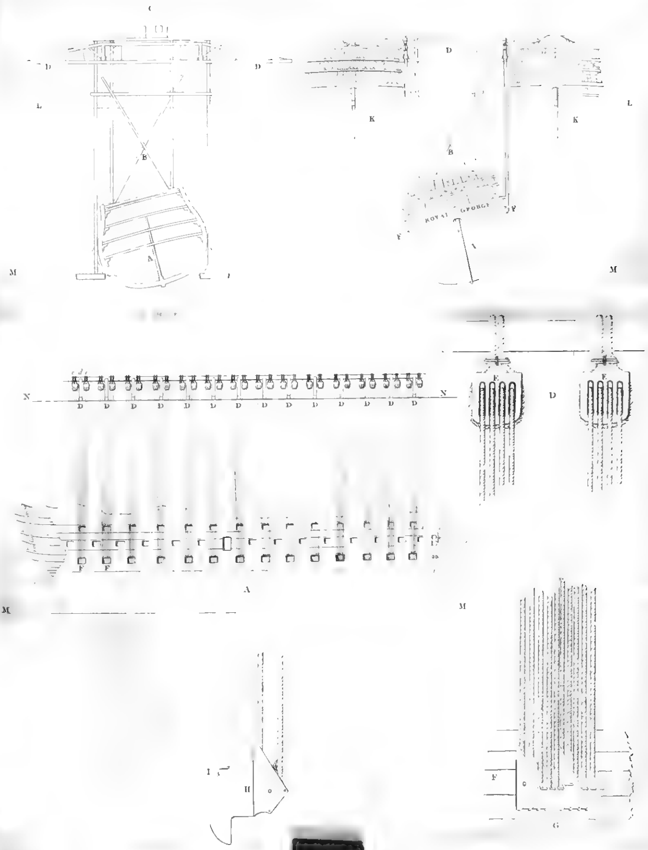
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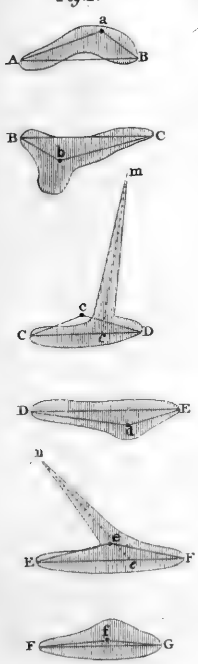

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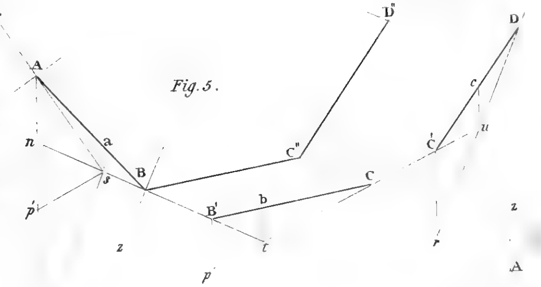

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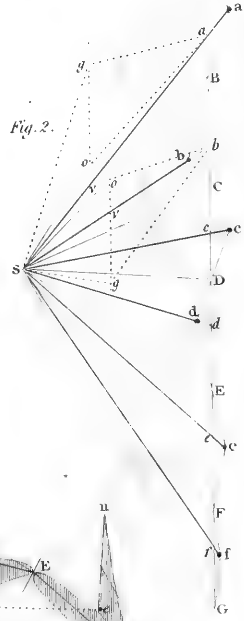

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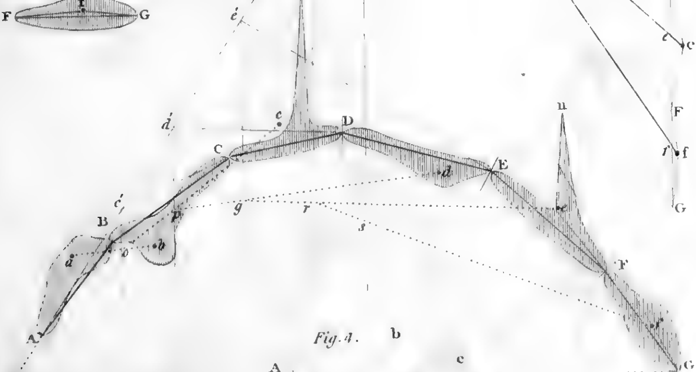

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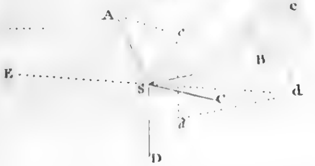

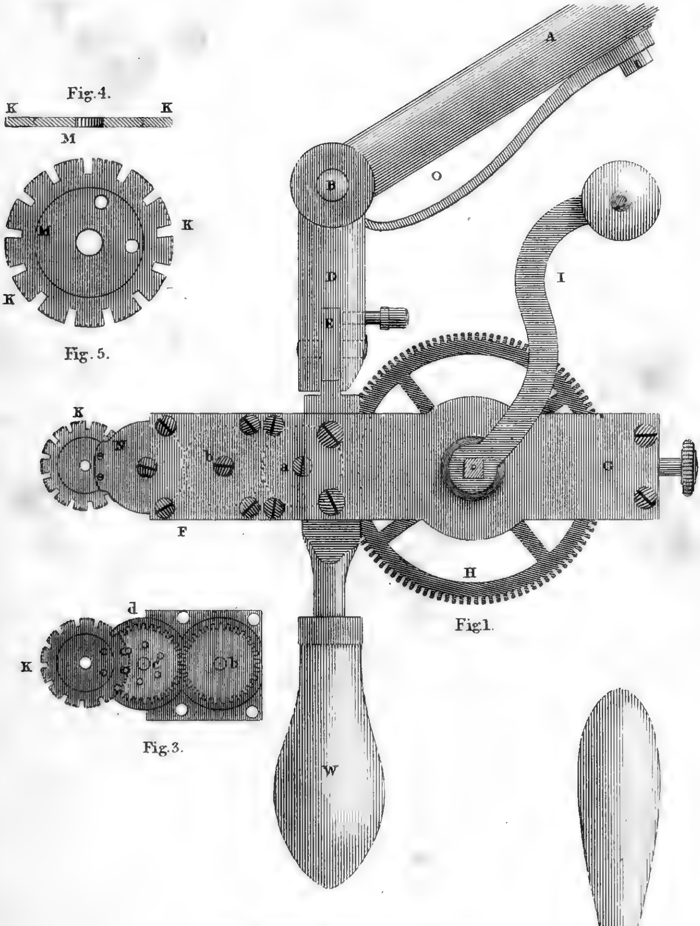

Mr. Thos. Machell's Annular Saw.

J. Porter sc.

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